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The Impact of Localized Economic Development on Crime: Evidence from West Virginia

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Abstract

Deutsch

Das Programm “Assistance to Coal Communities” (ACC) wurde von der United States Economic Development Administration gegründet, um Entwicklungsfonds für Gebiete, die vom Niedergang der Kohleindustrie betroffen sind, bereit zu stellen. West Virginia qualifizierte sich als einziger Bundesstaat als Ganzes für den Fonds. Basierend auf Vorfallmeldungen, die von lokalen Polizeibehörden an das FBI übergeben wurden, wurden die Auswirkungen der ACC auf Kriminalitätsraten auf Landkreisebene in West Virginia zwischen 2014 und 2022 analysiert. Die Anwendung des Differenz-von-Differenzen-Modells von Callaway und Sant’Anna belegt eine Senkung der Gesamt- und Eigentum-Kriminalitätsraten in den untersuchten Landkreisen. Dies stimmt mit den Erwartungen der bestehenden Literatur überein. Überraschenderweise zeigte sich darüber hinaus eine Senkung der Gewaltkriminalität. Zusammen mit dem ländlichen Fokus der Studie liefert dieses Ergebnis einen neuartigen Beitrag zu einem Forschungsgebiet, das sich bis dato weitgehend auf städtische Umgebungen konzentrierte.

English

The Assistance to Coal Communities (ACC) program was established by the United States Economic Development Administration to provide development funds to areas negatively impacted by the decline of the coal industry, with West Virginia being the only state designated eligible for funding in its entirety. Using incident reports submitted by local law enforcement agencies to the FBI, this paper estimates the effect of ACC funding on county-level crime rates in West Virginia between 2014 and 2022. Employing the Callaway-Sant’Anna difference-in-differences framework, results indicate that treated counties saw reductions in total and property crime, consistent with existing literature. More surprisingly, treated counties also exhibited a decrease in violent crime. This finding, combined with the study’s rural focus, offers a novel contribution to a field largely centered on urban environments.

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Abbreviation	**Meaning**
ACC	Assistance to Coal Communities
EDA	United States Economic Development Administration
FBI	Federal Bureau of Investigations
NIBRS	National Incident Based Reporting System

Introduction

The United States has one of the highest rates of crime amongst developed nations. This increase has been surrounded by heated debate assessing the origin of this phenomenon, most of which can be reduced to two mainstream schools of thought on why crime happens.

The first is that there is something fundamentally wrong with the individuals who commit crime — criminals are either inherently bad people lacking empathy or were raised under morally corrupting conditions. The second is that crime is the product of economic desperation (Ludwig 2025).

The rationale that a propensity to commit crime is something inherent to certain individuals informed the Super Predator Myth, which motivated much of the thought on crime policies enacted both at the federal and state level during the 1990s and early 2000s. Super-predators, a term coined by criminologist John DiLulio, referred to young men raised in dire poverty with little parental involvement who could not be controlled by law enforcement and committed violent crimes in "wolf packs" for sport (DiLulio 1995). Largely out of fear of super-predators, the 1990s saw the dismantling of protections for youths in the criminal justice system. Many states adopted policies that let minors be tried as adults and made punishments much harsher. The concept is now regarded as misfounded at best and a scare tactic aimed at dehumanizing young disadvantaged men at worst.

If economic desperation is the culprit, it would follow that improving economic circumstances should lower crime rates. Most studies within the field find that in practice this is partially true. As I will explain in the Effects of Economic Policy on Crime section, typically when economic conditions improve, there is often a drop in the rate of property crime but not necessarily violent crime (Ludwig and Schnepel 2024).

Operating off the assumption that crime is at least partially driven by economic desperation, it is my intention in this study to investigate whether policy levers, specifically economic development programs, can have an impact on crime rates.

With this in mind, it is important to distinguish that not all crime is considered equal. The societal costs associated with crime are made up of the direct financial impact on the victim, the costs incurred by law enforcement and the criminal justice system investigating and prosecuting, and the intangible costs to the victim, namely psychological distress. The exact numbers for the societal cost of different sorts of crime range quite a bit depending on estimate methodology, but without fail the costs associated with violent crimes are significantly higher than other types of crime.

To give just one example, in The Costs of Crime and Justice, Mark A. Cohen estimates the social cost of an incident of murder at roughly \$7.9 million, assault at \$25.3 thousand, robbery at \$30.7 thousand, and household burglary at \$3.1 thousand. These estimates are widely used in policy analysis due to their comprehensive inclusion of both direct and intangible victim costs. It can be potentially jarring to think about crime in these terms, but crime-costing allows for easier policy cost-benefit analysis. Cohen most recently estimated that the total cost of all crime in the United States in 2018 was roughly \$2.1 trillion annually, of which violent crime accounted for roughly 83% or \$1.7 trillion (Cohen 2020).

The issue of crime in America has long been an extremely hot button issue politically. During the 2024 election cycle roughly 60% of Americans reported that reducing crime should be a top priority for lawmakers (Gramlich 2024) and 64% felt that crime was on the rise nationally despite long term trends in national crime rates being on the decline (Gallup 2024).

During the summer of 2025, President Trump deployed the National Guard to Washington DC, Memphis, New Orleans, and Chicago for the stated purpose of combating rampant crime and threatened to do the same in New York, Portland, and Baltimore[1] (Face The Nation 2025). This move was controversial within the targeted cities and nationally. Opinions were divided between those that believed this was a purely politically motivated crackdown on liberal cities and those that believed the crime situation warranted these measures. Trump's critics were quick to note that crime rates, and violent crime rates more specifically, were actually down in the cities where troops had been deployed, and that sending in the national guard would not have his desired effect.

So if crime rates are down, how could rampant crime be a plausible justification? There is

[1]National Guard troops were also deployed to other cities however the stated justification in this case was to combat illegal immigration, not crime. In the cases of Memphis and Chicago immigration was also a motive given by the Trump Administration, but crime was emphasized as a primary justification.

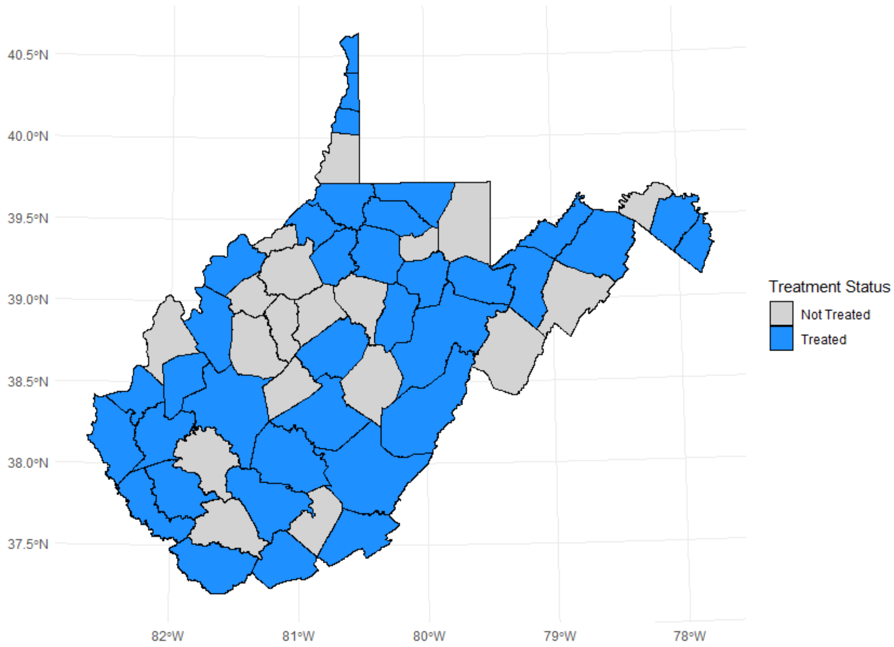
a well documented disconnect between actual crime rates and the public's perception of crime (Tonry 2011). Critics were correct to point out the drop in crime rates, but Trump's actions were still politically feasible because he capitalized on very real fears, held by many people nationally.

Beyond recent political discourse, conversations around crime in the US are still largely limited to crime in cities. By the same token, the vast majority of academic studies on crime and economics take place in urban environments, leaving a significant gap with regards to crime in more rural environments (Donnermeyer 2007).While it is true that the urban crime rates are much higher than rural crime rates, crime does not just occur in cities; America's crime problem is a widespread one.

I intend to assess the effect of localized economic development funding on crime in a rural environment, namely West Virginia. West Virginia is located in the Appalachian region of the United States and as of 2024, the state had a population of roughly 1.7 million, 55% of whom live in rural communities, making West Virginia the third most rural state in the country. Additionally, West Virginia has the second lowest GDP per capita out of all fifty states at $61,314 (World Population Review 2026b). This is by no means a new phenomenon, West Virginia has persistently lagged behind the other states economically. Historically the state's economy has been reliant on coal mining, which has long been a fraught industry. Mechanization in mining in the 1950s and 1960s prompted labor force downsizing and out-migration. In addition, the Appalachian mountain range has little land suitable for agriculture or industrial development. These trends only intensified with the shift away from coal as an energy source, resulting in West Virginia's now chronic economic struggle.

In 2014, the United States Economic Development Administration (EDA) launched the Assistance to Coal Communities Program to provide aid to areas impacted by the shift away from coal. The EDA identified 25 regions nationally where investment would be prioritized based on their dependence on coal mining and related industries. These regions were spread out across the eastern United States, mostly scattered throughout the South and Mid-Atlantic states. West Virginia, however, was the only state to be given priority in its entirety, including metropolitan regions (Collins et al. 2023). Between 2014 and 2022, 35 of West Virginia's 55

Figure 1: West Virginia Counties: Treated vs Not Treated



counties received funding through the Assistance to Coal Communities Program (Figure 1).

The scope of the Assistance to Coal Communities Program (ACC) was to provide grants to assist with technical planning, workforce development, entrepreneurship, and infrastructure projects. The EDA accepted applications from state and local governments, academic institutions, nonprofit organizations, tribal councils, and EDA district organizations. While the EDA does not publish a complete list of grant recipients, their annual reports do give some details on a selection of funded projects.

For example, one project the EDA highlighted in an annual report was Refresh Appalachia: Agricultural Entrepreneurship Development. This project provided agriculture training and workshops in areas with shortages of skilled agricultural workers. Another highlighted grant provided public works funding for the Bluefield Commercialization Station, a project that converted a disused railroad freight station into a mixed-use business incubator focusing on manufacturing (Economic Development Administration 2016).

It should be noted that the Assistance to Coal Communities Program was never explicitly intended to combat crime, though it is exactly the kind of intervention that may well have an indirect but tangible impact. The program was designed to support projects that foster economic diversification and create new jobs, which, following the logic that economic desperation pushes people to commit crime, should in turn drive down crime.

In this study I will assess the effect of ACC funding on county level crime rates in West Virginia between 2014 and 2022, using a staggered difference-in-differences design. First, I will delve into the existing literature in the field and discuss the effects of different economic interventions on crime. I will proceed by describing the local crime data, outlining my DiD approach utilizing a Callaway-Sant'Anna framework, and finally conclude with results and a breakdown of crime types and their implications.

Effects of Economic Policy on Crime

In addition to asking the explicit question of how the policies affect crime rates, studies on the effect of economic interventions ask an implicit question of why people commit crime. Operating off the assumption that economic desperation is what drives people to commit crimes, programs that alleviate that desperation, like direct transfers or jobs programs (by far the most common policies studied), should be effective in driving down crime rates. However, per the existing literature, it is not quite so straightforward.

The framing presented by Ludwig and Schnepel in their 2025 paper "Does Nothing Stop A Bullet Like A Job? The Effects Of Income On Crime" is particularly useful in tackling this issue; there is a difference between crime and the crime problem. While of course it would be preferable if we could reduce crime rates across the board, realistically when people talk about the crime problem, especially in the United States, they are talking about violent crime. Ludwig and Schnepel outline how the motivation behind property crime and violent crime are different and thus respond differently to policy intervention. Violent crimes tend to be more resistant to economic interventions since they are typically not economically motivated.

With that said, there are a few exceptions. When it comes to job training programs, there is evidence that youth summer job programs and funding public education can reduce violent crime rates (Lochner and Moretti 2004). Logically, one would expect programs like these where interventions target young people to be more successful, since criminal behavior tends to increase with age, peaks around late adolescence, and then decreases as individuals enter early adulthood (Farrington et al. 2013). It follows that providing opportunities, reducing economic desperation, and keeping young people who may otherwise get involved in criminal activity occupied would drive down crime rates.

Additionally there is evidence that in-kind transfers through government provided health insurance for households (Medicaid in the US) reduce violent crime rates. From 2014 to 2016, states had the opportunity to opt into an expansion of Medicaid. The expansion lifted eligibility requirements that largely restricted Medicaid access to the elderly, disabled people, and households with children, extending coverage to low income households more broadly. Multiple studies conducted after the expansion found that violent crime rates in states that opted into expansion significantly decreased relative to states that did not. The consensus is that the driving force behind the decrease was the sharp decrease in violent crimes committed by low income young adults, a demographic largely excluded from access to medical and mental health care prior to the expansion (Vogler 2020; He and Barkowski 2020; Simes and Jahn 2022).

The majority of studies in the field examining direct to household transfer programs found that transfers typically lead to declines in property crime rates, but have little to no effect on violent crime rates. These claims are backed up by the findings of Foley (2011); Watson, Guettabi, and Reimer (2020); and Deshpande & Mueller-Smith (2022), who looked at how crime rates responded to welfare payment timing, small scale universal basic income, and access to Supplemental Security Income.

There is also a wealth of literature studying the effect of in kind transfers, mainly through housing assistance and food subsidy programs. Studies conducted by Tuttle (2019), Luallen, Edgerton, and Rabideau (2018), Mueller-Smith, Reeves, Schnepel, and Walker (2023), and Carr and Packham (2019) all found that increased access to food subsidy programs in the US was associated with a decrease in property crimes, but had no effect on violent crimes. Additionally, two separate studies, one conducted in Chicago (Jacob et al. 2015) and the other in Houston (Carr and Koppa 2020), both found that housing vouchers had no detectable effect on crime.

Another common approach to combatting crime through economic mitigation is jobs programs. Much like transfer programs, jobs programs often have a mixed track record, only see results with regards to property crime, and have only really been studied within the context of cities. One study looking at a subsidized job program for people recently released from prison in New York found a mild reduction in recidivism for minor crimes, but no effect on more serious, violent crimes (Redcross, Millenky, Rudd, and Levshin 2012).

In Milwaukee, a similar job training/re-entry program saw a modest decline in re-arrests, though the types of crimes were not disaggregated in this study (Cook et al. 2015). Another study looking at the effect of job training on recidivism found that training programs seemed to be effective for older criminal offenders, but not younger offenders (Uggen 2000). Considering criminal behavior tends to decrease with age anyway, these results do not speak to an overwhelming success.

This encompasses most policy mechanisms used to address economic desperation as a root cause of crime, and overall, their effects have been somewhat underwhelming. However, the ACC is not a jobs or transfer program, rather it was designed to fortify macroeconomic conditions broadly.

Of the literature I reviewed, perhaps the most similar to my own research is that of Johnson, Kantor, and Fishback (2007), which examines the impact of the New Deal on crime. The New Deal, a package of economic, social, and political programs and reforms, was crafted in response to the Great Depression. Like the Assistance to Coal Communities Program, was never intended to directly impact crime, though President Franklin Roosevelt argued in defense of the New Deal, "crime cannot be held in check by a good police system alone... through a broad program of social welfare, we struck at the roots of crime."

The authors estimated the impact of relief spending on crime rates in 81 cities across the US from 1930 through 1940 and found that New Deal spending was indeed associated with a drop in property crime. A 10% increase in relief spending during the 1930s resulted in an approximately 1.5% decrease in property crime. In other words, for every additional $5,227 (roughly $122 thousand adjusted for inflation) allotted by the New Deal, one property crime was prevented (Johnson et. al 2007).

Of course, this is a historical study and its application today is limited. The political and economic situation in the US today is completely different from how it was in the 1930s and

1940s and the crime reporting data used in this study is much more detailed now, both in terms of the number of law enforcement agencies publishing data and in terms of the crimes reported.[2] Additionally, the distribution of New Deal money was in part politically motivated, areas more loyal to President Roosevelt's Democratic Party were often rewarded with more genius funding. Furthermore, this study looks exclusively at cities, making my focus on rural areas an added element of novelty.

Finally, in a 2025 paper, Brittany Street examined the effect of the fracking boom on crime in counties in North Dakota. Street used administrative data to estimate the effect of new job opportunities stemming from the uptick in manufacturing and fracking on criminal behavior. Street's work stands out in large part because of her focus on rural communities and her evaluation of the effect on rural residents already in the area prior to the fracking boom versus residents who moved in during the boom. Her analysis found that while there was an uptick in crime in fracking counties relative to non-fracking counties, that uptick was driven by individuals who moved in during the fracking boom and that during the same time frame there was a decrease in criminal behavior among long-time residents. Additionally, Street found that when disaggregated, there was a significant decrease in property crimes and no significant effect on any other type of crime (Street 2025).

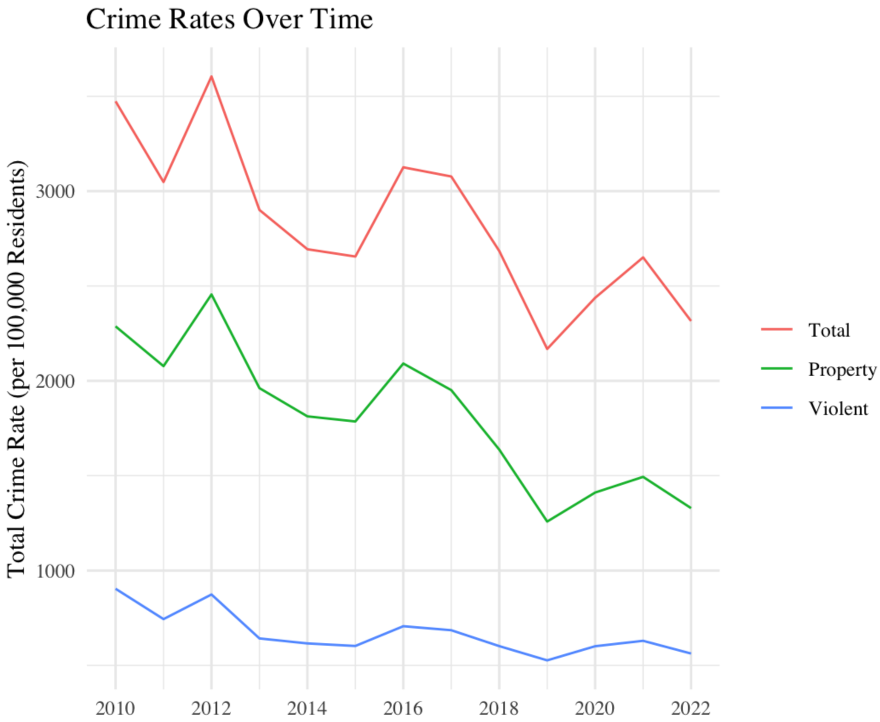
In line with the literature, it is my expectation that violent crime will not respond to EDA funding, but property crime rates will drop. This sort of response would be consistent with the findings illustrated by Johnson et al. (2007) and Street (2025). I also expect to see a drop in the overall crime rate, though it will not be as pronounced as the drop in property crime, as the overall rate will be buffered by the consistency in the rates of other crime types.

Data

The Federal Bureau of Investigation (FBI) annually releases data from the National Incident-Based Reporting System, or NIBRS, a comprehensive account of crime across the United States using reports submitted by individual law enforcement agencies. Using these

[2] Just 7 crimes were reported nationally in the 1930s and 1940s, whereas today 38 crimes are reported.

Figure 2: West Virginia Crime Rates Over Time



records, I collected data on crime from 2010 to 2022 reported by West Virginia police departments and aggregated them to the county level. The time frame was limited by the availability of NIBRS data. While NIBRS has existed since the 1980s, it was a secondary system where reporting was voluntary until it replaced the Summary Reporting System as the national standard in 2021. The Summary Reporting System, however, was not as detailed and comprehensive, so data from this system could not be used to supplement the NIBRS data.

I limited the law enforcement agencies whose data I looked at to city agencies, county agencies, and university/college agencies, excluding state agencies, federal agencies, and regional special agencies since they do not report with specificity where crimes took place, so incidents could not be assigned to treatment or control counties.

I created two separate flags for violent crime and property crimes.[3] As is illustrated in Figure 2, the rate of property crime is much higher than the rate of violent crime and all crime fell across West Virginia between 2010 and 2022.

Within the category of total crimes, there are infractions that are neither violent nor property

[3]The table of crimes and their categorization can be found in the appendix

crimes, such as traffic violations, prostitution, or disorderly conduct to list a few. Infractions like these are often referred to as crimes against society. These are crimes where there is no direct victim and are detected through police activity rather than victim reports. I chose not to create a separate flag for this category and elected to have these incidents only count towards the number of total crimes. I opted for this route since it is standard practice, the vast majority of the literature I reviewed was limited to violent and property crimes, and because such offenses impose comparatively limited measurable social costs (Cohen, Mark A. 2020).

West Virginia has the highest rate of drug use in the US with an estimated rate of 6,271 per 100,000 residents in 2022 and the highest rate of opioid and amphetamine use (World Population Review 2026a). Consequently there is a high level of drug crime in the state. However, the vast majority of drug crimes are recorded by state and national level law enforcement, which do not publicly provide any information on where the offenses occurred. With data on drug crimes available exclusively through local law enforcement agencies, less than 10% of drug crimes in the state are accounted for, therefore, I chose to exclude drug crime from my analysis as a separate disaggregation. So while drug crimes are indeed contributing to the total crime rate, what is available in the data is by no means a complete picture of the illicit drug market in West Virginia.

For incidents where multiple crimes were committed, say an aggravated assault and a simultaneous theft, each crime is recorded separately with a common incident number, indicating the two occurred at the same time. Since the two are recorded separately, the incident would count towards both the total number of violent and property crimes. I followed the FBI's Uniform Crime Reporting classification system when categorizing crimes by type, with the exception of robbery. The FBI classifies robbery, defined as "the taking or attempting to take anything of value from the care, custody, or control of a person or persons by force or threat of force or violence and/or by putting the victim in fear" (Federal Bureau of Investigation 2018), as a violent crime, but seeing as theft is part of the crime, I chose to categorize robbery as both a violent and property crime.

I then collapsed the data sets for each year by reporting agency and crime to obtain the number of each crime committed under the jurisdiction of the reporting agency in a given year. Reporting agencies were then matched to the county in which they are located, further collapsing the data.

Furthermore, to control for other factors that may have had an impact on crime rates during the time period of interest, I gathered information on the unemployment rate, average household income, and the number of police officers employed in the county per 100,000 residents. I obtained the county level unemployment rates from the Bureau of Labor Statistics, the average income level from the Census Bureau, and the number of officers per hundred thousand residents from the FBI's Law Enforcement Employees dataset (Federal Bureau of Investigation 2025). These were all common control variables in the literature reviewed.

It should be noted that it is not mandatory for law enforcement agencies to report their records to the FBI, and until 2021 NIBRS was not the primary reporting system, as previously mentioned. Consequently the NIBRS data are rather inconsistent and the panel is unbalanced, though as time progresses more agencies report more regularly. Of course, this is to be expected with data obtained outside of an experimental environment, but the limitations of the data should be acknowledged.

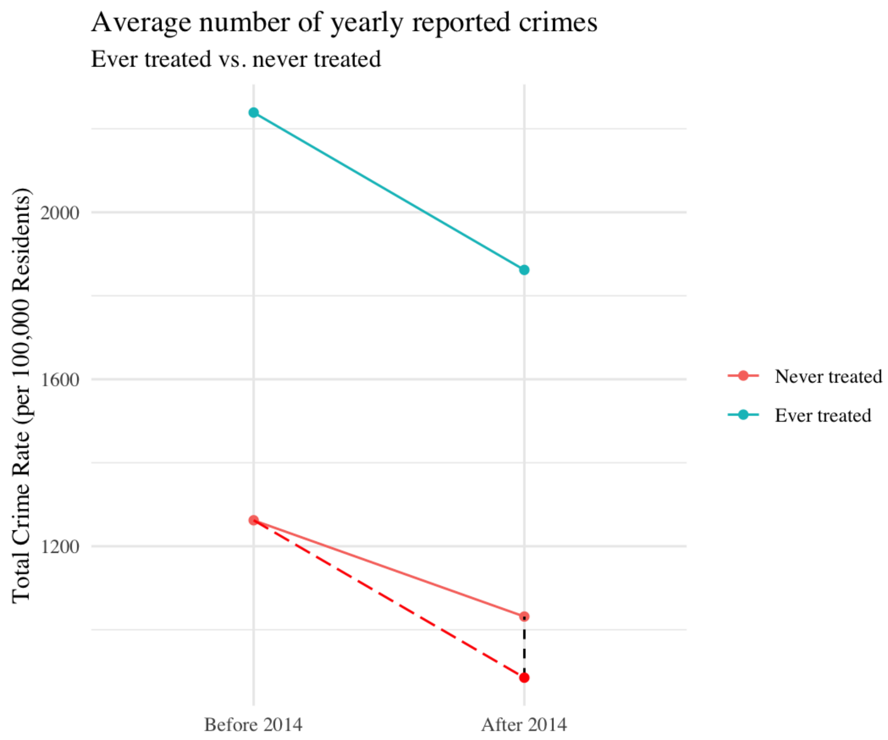
Methodology

To estimate the effect of EDA funding on county level crime rates, I employed a staggered difference-in-differences design using the Callaway and Sant'Anna (2021) framework, which accounts for variation in treatment timing across counties. Before delving into this approach, it is useful to first consider what a simple difference-in-differences estimate would suggest. Without controlling for other variables, Equation 1 indicates that the average number of total crimes decreased both in counties with and without intervention.

$$\beta_{DID} = (Y_{t_1,D} - Yt_0, D|D = 1) - (Y_{t_1,D} - Yt_0, D|D = 0) \quad (1)$$

In Equation 1, t_1 refers to the period after 2014 and t_0 refers to the period before 2014. The variable D is used to denote whether counties ever received treatment or not and Y represents

Figure 3: Average Crime Rates In Treated vs Untreated Counties



the average rate of total crimes per 100,000 residents. After the first counties were treated in 2014, the average annual crime rate in treated counties decreased by roughly 146 crimes per 100,000 residents more than the average number of crimes in non-treated counties, as given by Table 1. This difference can be seen clearly in Figure 3, which shows that while the total crime rate before and after treatment was higher in treated counties than untreated counties, the decrease over time was more dramatic in treated counties.

Table 1: Simple Difference-in-Differences Estimate

	Before 2014	**After 2014**	**Difference**
Treatment	2239.013	1861.945	-377.069
Control	1262.242	1031.589	-230.653
Difference			-146.416

However, this approach fails to capture the dynamics of the implementation of the Assistance to Coal Communities Program and the individual characteristics of the counties that may impact the crime rate. Under ideal experimental conditions, counties would be treated at the same time. However, due to the nature of the program, counties received treatment at various points between 2014 and 2022, so a staggered difference in difference approach is necessary in this case. I opted specifically for the Callaway Santa'Anna framework to assess the effect of the treatment. In the Callaway Sant'Anna framework, the composition of treatment groups changes over time to account for ongoing treatment and the control group is made up of individuals not yet treated and individuals who never receive treatment. For example, if a county was treated in 2017, it could be a part of the control cohort in 2014 and the treatment cohort for 2017 onwards.

This could theoretically be employed with a treatment cohort for every year since the program's rollout. However, due to data availability limitations and the size of some of those treatment cohorts (Table 2), I condensed the cohorts down to two: those treated in the first year of the program, 2014, and those treated at some point between 2014 and 2020 (referred to as the 2020 cohort).

Table 2: Counties Treated by Year

Year of First Treatment	**Number of Counties**
2014	12
2015	3
2016	3
2017	2
2018	5
2019	6
2020	3
2021	1
Not Treated	18
Total	**53**

I chose 2020 as the cutoff since the panel extends only to 2022, and counties treated after 2020 would have insufficient post-treatment observations to estimate meaningful effects.

I first ran the following equation to obtain the average treatment effect for each cohort in each year of the time frame conditioned upon county factors Equation 2:

$$ATT_{g,t} = [E(Y_{it}|G_i = g, X_i) - E(Y_{it}|G_i > t, X_i)] \tag{2}$$

The variable G denotes the treatment timing for an individual county, while g refers to the specific treatment cohort for a given year. That is to say for a county treated in 2017, G will always equal 2017. For the 2014 treatment cohort, that county would not be included and would be considered part of the control cohort. That county would however be included in the 2020 treatment cohort. For counties that were never treated, the treatment year G can be conceptualized as infinity since G will always be greater than t, the time period, for these counties.

X_{it} represents the vector of observed covariates for county i at time t, specifically the unemployment rate, median income, and the number of police officers employed in the county per 100,000 residents. Once the average treatment effect on the treated, ATT, is calculated for each cohort and time period, those values are then weighted and aggregated, resulting in a single aggregate average treatment on the treated effect (Equation 3). The weight, ω, reflects the relative size of the treatment cohorts and the number of post-treatment years each cohort is contributing, since the number of countries receiving treatment yearly is not uniform.

$$ATT_{aggregate} = \Sigma_{g \in G} \Sigma_{t=2} \omega_{g,t} * ATT_{g,t} \tag{3}$$

I ran this equation three times, once using the total crime rate as the dependent variable and once using each of the disaggregated crime rates to find the effect on each type of crime and on crime overall.

The difference in difference methodology relies on the assumption that the treated individuals do not anticipate their treatment and change their behavior accordingly and that trends in crime trends in the treatment and control cohorts are parallel prior to treatment.

Since access to funds required an application to the EDA, to an extent the treatment group is a self-selecting one. However, the EDA did not grant funding to every application submitted, so an applicant group would have no cause to act as though they would receive funding until their application was approved. Furthermore, it would not be common knowledge to anybody in the county outside of the institutions applying for funding that there was even the possibility of receiving funds. On these grounds, it is safe to assume that the no anticipation requirement has been met.

To assess the validity of the parallel trends assumption, a joint pre-test evaluates whether all pre-treatment ATTg,t estimates are statistically indistinguishable from zero. Failure to reject the null hypothesis supports the parallel trends assumption. This pre-test was conducted for both aggregated and disaggregated crime rates

Table 3: Dynamic Treatment Effects – Total Crime (Callaway–Sant'Anna Estimator)

Event Time	Estimate	Std. Error	Lower CI	Upper CI
-9	-432.319	895.326	-2187.158	1322.521
-8	-48.287	1041.608	-2089.838	1993.264
-7	636.771	884.673	-1097.187	2370.729
-6	-571.173	502.260	-1555.603	413.257
-5	301.986	435.538	-551.669	1155.641
-4	-466.929	254.521	-965.791	31.933
-3	-171.743	424.982	-1004.708	661.222
-2	-188.424	499.603	-1167.646	790.797
-1	-567.454	394.521	-1340.715	205.807
0	68.284	427.324	-769.270	905.838
1	-64.575	243.624	-542.078	412.927
2	-89.574	303.413	-684.262	505.115
3	-1239.224	409.534	-2041.910	-436.537
4	-2012.076	511.061	-3013.756	-1010.395
5	-1783.878	581.124	-2922.881	-644.875
6	-204.126	1390.678	-2929.855	2521.602
7	-1728.567	823.070	-3341.783	-115.351
8	-3101.045	1384.099	-5813.879	-388.211

Tables 3, 4, and 5 show dynamic treatment effects estimated using the Callaway and Sant'Anna DID estimator for total, property, and violent crime rates respectively. Coefficients are expressed in event time as opposed to calendar time. Event time 0 corresponds to the year of adoption, negative values indicate years prior to treatment, and positive values indicate years after treatment.

Since there are two treatment cohorts, one treated in 2014 and one treated by 2020, the range of event times reflects the available pre- and post-treatment observations for each cohort within the 2010–2022 panel. For example, the 2020 cohort contributes long pre-treatment periods, while the 2014 cohort contributes longer post-treatment periods. As a result, the number of contributing observations varies across event time.

Pre-treatment coefficients (event times less than zero) are statistically indistinguishable from zero and display no systematic pattern, providing supportive evidence that treated and

Table 4: Dynamic Treatment Effects – Property Crime (Callaway–Sant'Anna Estimator)

Event.Time	Estimate	Std..Error	Lower.CI	Upper.CI
-9	-282.736	703.210	-1661.027	1095.555
-8	-68.826	829.687	-1695.013	1557.361
-7	485.585	796.146	-1074.862	2046.032
-6	-369.948	378.931	-1112.652	372.757
-5	230.522	276.390	-311.202	772.246
-4	-303.231	162.070	-620.888	14.425
-3	-209.343	320.985	-838.473	419.787
-2	-109.979	311.863	-721.231	501.273
-1	-463.854	292.478	-1037.110	109.402
0	101.778	222.169	-333.673	537.230
1	-0.736	154.575	-303.702	302.230
2	-83.812	183.413	-443.301	275.677
3	-889.933	366.141	-1607.569	-172.297
4	-1364.821	481.020	-2307.621	-422.021
5	-1177.388	474.872	-2108.138	-246.639
6	235.418	1039.732	-1802.457	2273.294
7	-1198.094	569.626	-2314.560	-81.628
8	-2302.267	1128.758	-4514.633	-89.901

not-yet-treated counties followed comparable trends prior to treatment.

While some pre-treatment estimates appear large in absolute terms, none have simultaneous confidence bands that exclude zero. The wide confidence intervals observed at early event times (particularly -9 through -7) reflect the fact that these periods are populated primarily by the 2020 treatment cohort between 2010 and 2012, when NIBRS reporting participation in West Virginia was substantially lower and more inconsistent than in later years. As a result, these estimates are based on fewer observations and carry considerably more uncertainty, and should be interpreted with caution. Crucially, none of these estimates are statistically significant, and they display no systematic pre-trend pattern, supporting the parallel trends assumption.

Taken together, the absence of statistically significant pre-treatment effects across all three outcome variables supports the conditional parallel trends assumption and provides confidence that post-treatment estimates reflect the effect of EDA funding rather than pre-existing differences between treated and control counties.

Table 5: Dynamic Treatment Effects – Violent Crime (Callaway–Sant'Anna Estimator)

Event.Time	Estimate	Std..Error	Lower.CI	Upper.CI
-9	-114.507	188.102	-483.187	254.173
-8	46.410	201.970	-349.451	442.271
-7	111.015	169.305	-220.822	442.852
-6	-161.735	117.981	-392.978	69.507
-5	82.572	104.493	-122.235	287.378
-4	-83.460	75.354	-231.154	64.234
-3	26.771	102.060	-173.267	226.810
-2	-80.724	112.666	-301.549	140.101
-1	-80.886	84.100	-245.722	83.951
0	28.795	111.150	-189.059	246.650
1	-28.167	61.777	-149.249	92.915
2	0.659	73.157	-142.729	144.048
3	-209.381	96.561	-398.640	-20.122
4	-412.141	113.065	-633.748	-190.535
5	-314.963	120.962	-552.048	-77.878
6	-118.859	246.684	-602.359	364.641
7	-250.716	180.744	-604.975	103.542
8	-497.461	304.065	-1093.428	98.506

Findings

The group time ATT table for overall crime (Table 6) illustrates the effects of the treatment in a more detailed way. The 2014 treatment cohort did see a marked decrease in the overall crime rate from 2017 through 2022, though these effects were sporadic. When aggregated this comes out to an overall average treatment effect of approximately -711. In other terms, on average counties that received treatment saw 711 fewer crimes per hundred thousand residents than counties that did not receive EDA funding. For reference, the difference is roughly 22% of the mean pre-treatment total crime rate, making a decrease of 711 quite substantial. To get a clearer view of what is driving this difference we look to the disaggregated rates.

The group time ATT table for property crime (Table 7) shows a pattern very similar to that seen for total crime. There is a decrease in property crime rates for the 2014 treatment cohort in the later years of the study, and here too there is a blip in 2020. The aggregated treatment effect comes out to a decrease of about 459 crimes. For reference, the difference is roughly 21% of the mean pre-treatment property crime rate, making this again a fairly substantial difference, though it is only significant at the 10% level.

As for violent crime (Table 8), the effects are smaller, though violent crime rates are also notably lower, as is typical. At the group time level, there were small, sporadic, mildly significant effects for the 2014 cohort and no effect on the 2020 cohort. This comes out to an aggregated average treatment effect of -124, which is about 16% of the mean pre-treatment violent crime rate. As mentioned in the literature review section, economic interventions do not typically have a noticeable effect on violent crime, so this result is surprising.

A commonality between all three crime rates is that the 2014 cohort only really started seeing results in 2017 and those results dropped off in 2020. The disruption in 2020 is expected since the Covid-19 Pandemic disrupted economic and crime patterns nationally. As for the effect kicking in in 2017, three years after initial treatment, this may come down to the development projects taking time to materialize into jobs and quality of life improvements, which then take further time to affect crime. Additional information on the kinds of development projects funded in this initial cohort could be potentially illuminating,

Table 6: Group-Time Average Treatment Effects: Total Crime Rate

Cohort	Calendar Year	ATT(g, t)	95% Confidence Interval
2014	2011	-107.902	$[-2502.417,\ 2286.612]$
2014	2012	-858.193	$[-3177.068,\ 1460.683]$
2014	2013	-1336.484	$[-3664.467,\ 991.499]$
2014	2014	-11.419	$[-1184.191,\ 1161.353]$
2014	2015	-173.502	$[-895.684,\ 548.679]$
2014	2016	-152.498	$[-1089.155,\ 784.158]$
2014	2017	-1239.224^{***}	$[-2069.022,\ -409.425]$
2014	2018	-2012.076^{***}	$[-3048.348,\ -975.803]$
2014	2019	-1783.878^{***}	$[-2999.038,\ -568.718]$
2014	2020	-204.126	$[-3082.522,\ 2674.269]$
2014	2021	-1728.567^{**}	$[-3409.987,\ -47.147]$
2014	2022	-3101.045^{**}	$[-5821.148,\ -380.942]$
2020	2011	-432.319	$[-2216.229,\ 1351.592]$
2020	2012	-48.287	$[-2056.241,\ 1959.666]$
2020	2013	636.771	$[-1228.662,\ 2502.204]$
2020	2014	-571.173	$[-1597.035,\ 454.689]$
2020	2015	301.986	$[-461.581,\ 1065.552]$
2020	2016	-466.929^{*}	$[-988.954,\ 55.096]$
2020	2017	-206.565	$[-554.110,\ 140.980]$
2020	2018	176.904	$[-201.174,\ 554.983]$
2020	2019	-147.983	$[-623.000,\ 327.034]$
2020	2020	111.758	$[-1062.648,\ 1286.165]$
2020	2021	-5.161	$[-585.272,\ 574.951]$
2020	2022	-55.252	$[-762.617,\ 652.114]$
Aggregated ATT			**95% Confidence Interval**
-711.186^{**}			$[-1324.608,\ -97.765]$

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

though this is not publicly available.

Additionally, in comparison to the 2014 cohort's large Group Time Average Treatment Effects, the aggregated ATT is perhaps unexpectedly small. This is due to the weighting. Weights are determined by the size of each cohort and the number of post-treatment years observed from each one. The 2020 cohort is nearly double the size of the 2014 cohort, 23 counties versus 12 counties, which accounts for the discrepancy.

Table 7: Group-Time Average Treatment Effects: Property Crime Rate

Cohort	Calendar Year	ATT(g, t)	95% Confidence Interval
2014	2011	-209.066	$[-2020.712,\ 1602.580]$
2014	2012	-470.522	$[-2145.543,\ 1204.500]$
2014	2013	-1136.381	$[-2844.516,\ 571.753]$
2014	2014	-104.694	$[-974.124,\ 764.737]$
2014	2015	-167.515	$[-802.210,\ 467.181]$
2014	2016	-91.398	$[-822.372,\ 639.576]$
2014	2017	-889.933^{**}	$[-1625.344,\ -154.522]$
2014	2018	-1364.821^{***}	$[-2319.357,\ -410.285]$
2014	2019	-1177.388^{**}	$[-2126.652,\ -228.125]$
2014	2020	235.418	$[-1778.276,\ 2249.113]$
2014	2021	-1198.094^{**}	$[-2346.929,\ -49.259]$
2014	2022	-2302.267^{**}	$[-4531.234,\ -73.300]$
2020	2011	-282.736	$[-1747.273,\ 1181.801]$
2020	2012	-68.826	$[-1685.307,\ 1547.655]$
2020	2013	485.585	$[-978.720,\ 1949.890]$
2020	2014	-369.948	$[-1119.292,\ 379.396]$
2020	2015	230.522	$[-298.634,\ 759.677]$
2020	2016	-303.231^{*}	$[-652.433,\ 45.970]$
2020	2017	-209.494^{*}	$[-449.008,\ 30.020]$
2020	2018	86.681	$[-143.129,\ 316.490]$
2020	2019	-97.022	$[-391.854,\ 197.811]$
2020	2020	214.400	$[-384.234,\ 813.033]$
2020	2021	90.234	$[-271.176,\ 451.645]$
2020	2022	-79.674	$[-459.714,\ 300.366]$
Aggregated ATT			**95% Confidence Interval**
-458.501^{*}			$[-939.965,\ 22.962]$

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Group-Time Average Treatment Effects: Violent Crime Rate

Cohort	Calendar Year	ATT(g, t)	95% Confidence Interval
2014	2011	134.549	$[-374.768,\ 643.866]$
2014	2012	-322.959	$[-875.949,\ 230.031]$
2014	2013	-229.972	$[-702.643,\ 242.698]$
2014	2014	48.983	$[-191.730,\ 289.695]$
2014	2015	-6.860	$[-129.751,\ 116.031]$
2014	2016	-5.615	$[-211.819,\ 200.588]$
2014	2017	-209.381^{**}	$[-393.798,\ -24.964]$
2014	2018	-412.141^{***}	$[-630.378,\ -193.905]$
2014	2019	-314.963^{**}	$[-560.661,\ -69.264]$
2014	2020	-118.859	$[-562.257,\ 324.539]$
2014	2021	-250.716	$[-623.642,\ 122.209]$
2014	2022	-497.461^{*}	$[-1069.542,\ 74.620]$
2020	2011	-114.507	$[-486.884,\ 257.869]$
2020	2012	46.410	$[-370.744,\ 463.564]$
2020	2013	111.015	$[-215.497,\ 437.527]$
2020	2014	-161.735	$[-380.346,\ 56.875]$
2020	2015	82.572	$[-113.692,\ 278.836]$
2020	2016	-83.460	$[-244.221,\ 77.302]$
2020	2017	-32.017	$[-130.664,\ 66.631]$
2020	2018	51.404	$[-37.153,\ 139.962]$
2020	2019	0.434	$[-98.249,\ 99.117]$
2020	2020	17.784	$[-301.254,\ 336.823]$
2020	2021	-39.788	$[-193.136,\ 113.560]$
2020	2022	4.082	$[-199.643,\ 207.807]$
Aggregated ATT			**95% Confidence Interval**
-124.129^{*}			$[-254.894,\ 6.636]$

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Conclusion

My findings regarding total and property crime are consistent with my initial hypothesis. Counties that received funding did indeed see a reduction in the overall crime rate, and when disaggregated the most dramatic reduction took place in the rate of property crime. However, the expectation that there would be no effect on violent crime was incorrect. Contrary to the findings of similar studies, there was indeed an observed decrease in violent crime rates in counties that received funding. These results are statistically significant, total crime at the 5% level and both property and violent crime at the 10% level, and are supported by the joint pre-treatment test, in which no pre-treatment coefficients were statistically distinguishable from zero across any of the three outcome variables, confirming that the parallel trends assumption holds. Given the evidence, I am able to reject the null hypothesis that receiving EDA funding would have no impact on total crime rates and property crime rates.

This raises the question, why is this effect evident when violent crime so rarely responds to economic interventions? Seeing as counties that received funding saw a significant decrease in violent crime, the Assistance to Coal Communities Program stands out from the many jobs training, convict re-entry, direct transfer, and in kind transfer programs in its effect on violent crime.

I suspect that economic development interventions had a larger marginal effect in West Virginia than they would in a more economically diversified state. Funding was provided to areas that are genuinely resource starved, meaning the same dollar amount of EDA funding may go further than it would elsewhere. Considering most economic intervention programs were studied in urban environments, where economic hardship is largely an issue of access and not an absolute lack of resources, it would follow that the impact would be different in rural environments. Cities are by nature more economically diverse. Opportunities exist in cities, but systems of exclusion block people from accessing them. In rural areas those opportunities just do not exist to begin with, so it would follow that funding to create opportunities would see results (Donnermeyer 2007).

Whether or not the effect exhibited can justify the continued funding or expansion of the Assistance to Coal Communities Program (or similar programs) in the future remains in question. In qualifying the success of the ACC's impact on crime rates, more attention should be paid to the type of crime rather than the scale of the reduction. While of course, a decrease in any type of crime is a desirable outcome, it is preferable to see a decrease in violent crime, especially from a policy cost-benefit point of view. The program's impact on violent crime rates is of particular note.

Circling back to Cohen's crime-costing model, violent crime costs the United States $1.7 trillion and makes up 83% of the cost of crime overall. Even a modest decrease is highly valuable. Of course, the observed effect on property crimes was more dramatic, but seeing as property crimes only account for a small share of the social costs of crime, it would be harder to justify funding a program like the Assistance to Coal Communities Program if the effect was only seen under this category.

Within the context of rural communities facing economic hardship, crime reduction can be seen as a valuable co-benefit, since the program was crafted with economic revitalization and diversification in mind, and it is my opinion that the program expansion would be a worthwhile endeavor in similar communities. Whether or not similar results would come about in urban environments remains to be seen, though I doubt the effect would be as pronounced.

The limitations that I ran up against in this study present a few interesting avenues for future research. As previously mentioned, in 2021 the FBI switched from the Summary Reporting System to the NIBRS system as a more detailed and comprehensive reporting system. Consequently, participation in the NIBRS has increased significantly in recent years, with coverage jumping from 49% to 82% of law enforcement agencies between 2021 and 2023. The limitation created by the unbalanced nature of the panel could be addressed by using more recent data. Administering similar treatment in a randomized control trial using up to date NIBRS data could provide an even clearer, more reliable picture of the effect of this sort of intervention, and would serve as a valuable basis for evaluating whether the ACC or similar programs merit continued funding.

Furthermore, while I approached treatment as binary, either a county received funding or it did not, counties did not receive identical grants. A dose-response analysis examining whether counties that received larger grants saw larger reductions in crime rates could be a valuable followup.

Finally, while the specific recipients of ACC funding are not made readily available to the public, a Freedom of Information Act request could be submitted to obtain a list of exactly which institutions received funding and what projects were supported. With this information, a spatial analysis could be conducted to test whether effects were concentrated around the investment sites or whether they are diffused more broadly since NIBRS data are available at the zip-code level. This approach could be particularly interesting with the crime type disaggregation utilized in this study as it might be expected that property crime effects would be more localized around investment sites whereas violent crime effects would radiate more broadly.

Crime in the United States has long been treated as a strictly urban problem and policy responses have largely reflected this assumption. The evidence presented here suggests that economic development funding, though never explicitly intended to combat crime, had a meaningful if modest impact on crime rates in one of the most rural states in the country. As conversations around crime in America continue to be dominated by debates over policing and punitive measures, which have yielded little of worth, it may be worth asking what other interventions can be designed to address the problem at the root and considering the specific needs of communities when crafting policy.

Appendix

Table 9: Crime Classifications

Category	Crime
Violent Crimes	Aggravated assault
	Fondling (Indecent Liberties/Child Molesting)
	Intimidation
	Justifiable homicide
	Kidnapping/abduction
	Murder/nonnegligent manslaughter
	Negligent manslaughter
	Rape
	Robbery
	Sexual assault with an object
	Simple assault
	Sodomy
	Statutory rape
Property Crimes	All other larceny
	Arson
	Burglary/breaking and entering
	Counterfeiting/forgery
	Credit card/automatic teller machine fraud
	Destruction/damage/vandalism of property
	Embezzlement
	Extortion/blackmail
	False pretenses/swindle/confidence game
	Hacking/Computer Invasion
	Identity Theft
	Impersonation
	Motor vehicle theft
	Pocket-picking
	Purse-snatching
	Robbery
	Shoplifting
	Stolen property offenses (Receiving, Selling, Etc.)
	Theft from building
	Theft from coin-operated machine or device
	Theft from motor vehicle
	Theft of motor vehicle parts/accessories
	Welfare fraud
	Wire fraud

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