

2022

Three Golden Balls: Pawn Shops and Crime

Bryan McCannon

West Virginia University, bryan.mccannon@mail.wvu.edu

Zachary Rodriguez

Syracuse University

Zachary Porreca

West Virginia University

Follow this and additional works at: https://researchrepository.wvu.edu/faculty_publications



Part of the [Public Economics Commons](#)

Digital Commons Citation

McCannon, Bryan; Rodriguez, Zachary; and Porreca, Zachary, "Three Golden Balls: Pawn Shops and Crime" (2022). *Faculty & Staff Scholarship*. 3112.

https://researchrepository.wvu.edu/faculty_publications/3112

This Article is brought to you for free and open access by The Research Repository @ WVU. It has been accepted for inclusion in Faculty & Staff Scholarship by an authorized administrator of The Research Repository @ WVU. For more information, please contact beau.smith@mail.wvu.edu.

Three Golden Balls: Pawn Shops and Crime

Bryan C. McCannon*
West Virginia University

Zach Porreca†
West Virginia University

Zachary Rodriguez ‡
Syracuse University

February 2, 2022

Abstract

We ask what the relationship is between pawn shops and crime. The dominant narrative is that pawn shops reduce the transaction costs of crime and, consequently, promote it. We explore the alternative where pawn shops address the financial distress of those in need, which reduces the incentive to engage in crime. We exploit two distinct policies affecting access to pawn shops – severe licensing fees implemented in London in the early 1800s and state variation in the classification of pawn shops as essential businesses during the Covid-19 pandemic in spring 2020. For each, employing a difference-in-difference identification strategy, we provide evidence that restrictions to pawn shop access *increases* property crime.

Keywords: Covid-19; essential business; Georgian London; Old Bailey; pawnbroker; pawn shop; property crime

JEL codes: K42; L51

1 Introduction

Since the Middle Ages, lenders accepting pledges of personal property have signaled to borrowers using three golden balls suspended from a bar.¹ By leaving valuable property at these shops as collateral, borrowers can quickly access liquidity, which enables them to meet their short-term obligations. Pawnbrokers play an important role within communities for people who need funds quickly, but do not qualify for traditional bank loans (Bolen et al., 2020). While pawnbrokers provide valuable financial services to their clients, there is concern that their shops may also facilitate crime.

What relationship might we expect to exist between pawn shops and crime? A dominant narrative is that pawn shops promote crime. Empirical investigations support this (d’Este, 2020; Kubrin and Hipp, 2016). By providing an avenue to easily liquidate durable, consumer goods, the argument is that pawn shops reduce

*Corresponding author. Department of Economics, John Chambers College of Business and Economics, West Virginia University, 1601 University Avenue, Morgantown WV 26506, bryan.c.mccannon@gmail.com. We thank Fran Bishop of the National Pawnbrokers Association for useful discussions on the industry.

†Department of Economics, John Chambers College of Business and Economics, West Virginia University, 1601 University Avenue, Morgantown WV 26506, zjp00003@mix.wvu.edu

‡Department of Entrepreneurship and Private Enterprise, Whitman School of Management, Syracuse University, 721 University Avenue, Syracuse, NY 13244-2450, zachary.rodriquez1@gmail.com

¹Different arguments exist over the history of the meaning of the symbol of three golden balls. In one, the Medici family of Florence, Italy may have been an early originator of the financial practice in Europe. These Lombard merchants initially used three gold coins as their symbol, which were later transformed to the three-dimensional balls. In another, Saint Nicholas, the patron saint of pawnbrokers, as Bishop of Myra gave three bags of gold coins to a man who did not have the ability to pay the dowry for his three daughters. See De Roover (1946) for an extensive history on its symbolism.

the transaction cost to property crime. This should encourage more crime.² Here, we question the validity of this narrative.

An alternative argument, which we explore, is that pawn shops provide two valuable services to the public: They provide credit for those who need temporary loans and act as a low cost method to sell assets. Thus, they relieve people’s financial pressures. Without pawn shops, those in financial distress may choose criminal activity. Under this argument, access to pawn shops reduces property crime.

A prominent example of research investigating pawn shops and crime is d’Este (2020). Specifically, he considers the relationship between the number of pawn shops operating in a U.S. county and its level of property crime. In a panel data set he finds that increases in the number of pawn shops are correlated with the amount of property crime. This is taken as evidence that pawn shops reduce the transaction cost of crime. In a second, noteworthy study Kubrin and Hipp (2016) look at block-level data in Los Angeles matching the number of pawn shops (and other fringe financial institutions) to crime. They too find a positive relationship between the number of pawn shops and crime.

Unfortunately, these studies do not have any exogenous shock to the pawnbroking industry. This is important as both the number of pawn shops and the volume of property crime can be driven by important omitted variables, such as the degree of financial distress experienced in a community.

We contribute to this research question by taking a two-pronged approach. First, potential substitution effects between differing outlets for stolen items cannot be evaluated when considering the total number of property crimes (and its subcomponents such as burglary, robbery, and larceny). In other words, it is not clear that pawn-induced crime and non-pawn crime co-move. To address this, we analyze a novel data set of crime in early 1800s London. Using a text analysis of criminal trials from the main courthouse in London, known as the Old Bailey, we search for terms related to pawn shops. This allows us to directly measure crime that involves pawn shops and separate it from crimes that do not involve pawn shops. This allows us to evaluate potential substitution effects.

We leverage two important policy changes in Georgian England. First, the Pawnbrokers Act of 1800 legitimized the pawnbroker industry legalizing their activities, standardizing interest rates, and regulating holding times for assets pawned. This created an era of pawn shop access in London. Second, the British Parliament in 1815 introduced severe licensing fees for pawn shops operating in London, with milder fees for those across the rest of the country.³ As high licensing fees can be expected to drive operators out of business, we explore the shock of reduced access to pawn shops after 1815 to the period of pawn shop acceptability during the 1800-15 period.

Here, we first show that the period of legitimized pawnbroking corresponds to more thefts relative to the

²Kubrin and Hipp (2016) also reference a sociological theory known as Social Disorganization that posits a destructive effect of pawn shops on local, social order. For example, increased debt could promote crime as a solution as borrowing creates addiction. Further, pawn shop customers will be carrying cash and, hence, create an opportunity for robbery.

³The motivation for the licensing fee is not clear. For one, debts from the Napoleonic War were at a peak (Napoleon’s surrender at Waterloo occurred in 1815) and the licensing fee may simply have been a new revenue source for the British government. Alternatively, anti-Semitism could have been a motivation for the law as a large number of pawnbrokers in London were Jewish. Finally, the licensing fee could have been motivated by concerns over rising crime.

period of restricted pawn shop access, as also documented in modern data by d’Este (2020) and Kubrin and Hipp (2016). Exploring the text, though, we show that this can be fully explained by increases in thefts unrelated to pawn shops, and that there is no change in the prevalence of references to pawn shops in trial testimony. Therefore, it is important to recognize that pawn shops are only rarely used in property thefts and that a correlation between an increase in the number of pawn shops and an increase in aggregate crime does not necessarily imply causation.

We continue with this data and explore a panel data set comparing the annual volume of theft-type crimes to the annual volume of crimes that are shown from the trial testimonies to be essentially unrelated to pawn shops. Using the latter as the control, we use a difference-in-difference estimation strategy and provide evidence that the closure of pawn shops led to an *increase* in the amount of thefts. This supports our alternative hypothesis that pawn shop access reduces crime.

Our second approach to the research question directly addresses the exogeneity of access to pawn shops. Moving to modern activity in the United States, we evaluate inter-state variation in the classification of pawn shops at the height of the Covid-19 pandemic during the spring of 2020. As the rapid spread of the Coronavirus threatened public health and the effectiveness of health care providers, state governors issued Stay-at-Home orders closing schools, public offices, non-essential businesses, and encouraged Shelter-at-Home practices (Hall and McCannon, 2021; McCannon, 2021). The objective of these orders was to mitigate Covid-19 related deaths.⁴ Pawn shops are typically classified as financial institutions. Consequently (and controversially), pawn shops were allowed to stay open. Six states did not allow this exception and forced pawn shops to close. Using the FBI’s monthly Uniform Crime Report data, we employ a difference-in-difference identification strategy to assess the impact of pawn shop closings on property crime.

With this identification strategy, we present additional evidence that the closure of pawn shops is associated with an increase in property crime. This occurs both countrywide, using a state by year panel data set, and using a county by month case study of New Mexico where intra-state variation exists. Thus, access to pawn shops can reduce the prevalence of property crime for those in financial distress.

Our empirical investigation covers two countries and restrictive policies centuries apart. Interestingly, both novel events provide similar results. In Georgian London, while the 1800 to 1815 time period is associated with more crime broadly and, consequently, more property crime, there is no change in the prevalence of pawn shops in the trial testimony of property crimes once they are restricted. Only omitted factors can explain the relative increase in thefts. Further, using the volume of unaffected crimes as a control, we show that the policy’s impact was to increase thefts. Covid-era U.S. data further suggests that our alternative narrative may more appropriately explain the relationship between property crimes and pawn shops as jurisdictions which shut down pawn shops experienced relative increases in property crime. Both results challenge the dominant narrative that pawn shops promote crime and support the alternative argument that pawnbrokers reduce financial pressures that otherwise lead to property crime.

⁴For a study evaluating the success of such order at limiting the virus’s spread see Engle et al. (2020).

One could also connect our work to research on other types of “fringe banking”. For example, whether payday lending has measurable improvements in borrower’s economic outcomes has received considerable attention (Stegman, 2007; Melzer, 2011). Research has investigated whether payday lending borrowers are aware of the financial consequences (Dobbie and Skiba, 2013); and its spillover effect on bankruptcy (Skiba and Tobacman, 2019), overdrafts (Morgan et al., 2012), foreclosures (Morse, 2011), liquor sales (Cuffe and Gibbs, 2017), and even military performance (Carrell and Zinman, 2014; Carter and Skimmyhorn, 2014) has been studied.⁵ While we do not explore the industry’s benefits or consequences on personal finances, we focus on its relationship to crime.

We first describe the regulatory environment in Georgian London and the data available in Section 2. Section 3 presents the empirical results using Old Bailey trials and exploits variation in policy over time. In Section 4 we turn to pawnbroking in the United States and detail the state variation in essential business classifications across the country. The empirical investigation of crime in the U.S. is presented in Section 5. Section 6 concludes.

2 Pawnbroking in London

2.1 Before 1800

The history of pawnbroking in London dates back to the early middle ages. One of the earliest instances of English pawnbroking recorded is King Edward III famously pawning his crown and jewels at 50% interest to fund a war with France in 1388 (Sumption, 1999). Pawnbroking was an integral and expensive part of life for London’s poor, with interest rates as high as 30% often charged (Brealey, 2013). In 1707 the Charitable Corporation was established with the stated aim of providing pawnbroker’s loans to London’s poor at interest rates no greater than 6% annually (Carr, 1913). Unfortunately, amid allegations of impropriety and fraud among shareholders, the Charitable Corporation collapsed (Nevill, 1732).

The first regulation of pawnbroking in England dates to 1603, and was driven by the perception that pawnbrokers dealt in stolen property (Swain and Fairweather, 2012). However, an alternative, more positive view of the pawnbroker was presented in a 1745 *Gentleman’s Magazine* article that stated that “greater numbers of pawnbrokers appear at the Old Bailey upon account of goods and felons which they have stopt, than on account of stolen goods which they have received” (Swain and Fairweather, 2012). Several acts of Parliament during the subsequent decades sought to impact the day to day activities of pawnbrokers through regulation of interest rates and setting minimum periods of holding pledged goods. However, the first substantial regulation came in 1785, when the first pawnbroker’s license was introduced (Penderel-Brodhurst, 1911).⁶

Early critics of the pawnbroking trade saw that most of the clientele were from lower-income groups, and that pawning introduced another mechanism of indebtedness into their already stressful lives (Minkes,

⁵For a thorough overview of research on these complementary financial markets see Bolen et al. (2020).

⁶The cost of the license was set at £10 in London and £5 in the countryside. A maximum interest rate at 0.5% monthly, and it set a maximum loan duration of one year.

1953). Further exacerbating tensions over the industry was the association between the pawnbroker industry and religion as early pawnbroking in England was conducted by Jewish residents. Anti-Semitism, usury laws, and concerns over adverse effects on the poor made for an industry forced to operate with uncertainty (Raymond, 1978).

2.2 The 19th Century

To resolve the legal uncertainty of pawnbrokers in London, Parliament passed the Pawnbroker's Act in 1800. Championed by Lord Eldon, who was toasted at pawnbroker dinners for years after (Coffey, 1966), the pawnbroker industry received legitimization. Importantly, the industry was standardized. Pawnbrokers could charge up to 20% interest per year. Pledges must be allowed to be redeemed for up to 15 months after, and loans can be granted for up to a year.

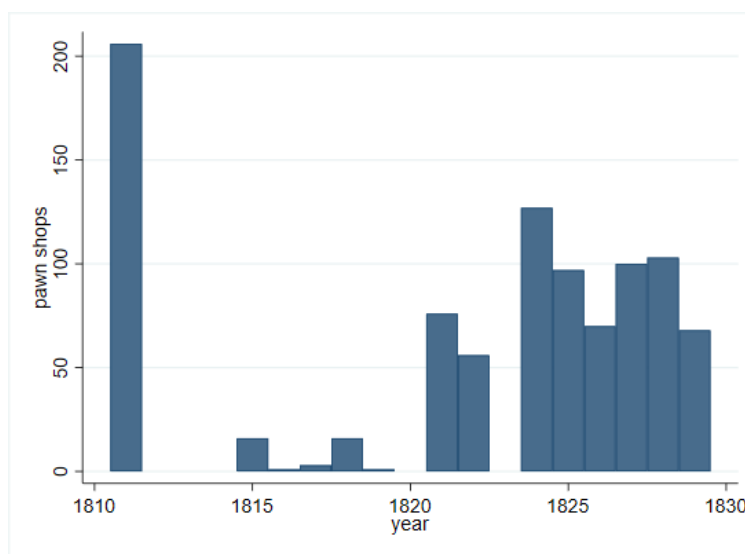
Pawn shops were free to operate under standardized guidelines. This persisted until a modification of the Pawnbroker's Act. In 1815 the British Parliament increased the licensing fee to £15 in London. A lower, but still severe, fee of £7.5 was imposed across the rest of the country. Further, major modifications to the industry's regulations did not occur until 1872.

Using the logic of endogenous entry into a Cournot market, theory predicts that a substantial increase in the fee, as a fixed cost, in an imperfectly competitive market will cause the equilibrium number of firms who compete in that market to reduce. Thus, we have a policy intervention in Georgian London which reduces the number of pawn shops operating. Consequently, exploring crimes in the city during the 1800-15 time period to years after 1815 provides an opportunity to assess the relationship between the number of operating pawn shops and the prevalence of property crime.

While standard economic theory, as stated, predicts a reduction in pawn shops, evidence from this period substantiates this belief. Using historical city and county directories in England over this time period provides us with information on the number of operating pawnbrokers.⁷ These directories were created as a general description of a city or county that would include information on local transportation, churches, schools, government offices, and businesses. Directories also include information on specific individuals who ran these institutions. While records are not available for every year, the directory for 1811 is, along with numerous years over the 1815 to 1830 time period. Figure 1 depicts the counts of registered pawnbrokers operating pawn shops in these directories.

⁷We use the UK, City and County Directories, 1766 - 1946 available through Ancestry.com to find information on pawnbrokers in England. One can search their online directory to find the specific occupation of an individual, and so we searched for the exact phrase, "pawnbrokers", and narrow our search to only within England. This query creates a data set of 940 registered pawnbrokers in England from 1811 to 1830. Ancestry.com. UK, City and County Directories, 1766 - 1946 [database on-line]. Provo, UT, USA: Ancestry.com Operations, Inc., 2013 (accessed December 2021).

Figure 1: Pawnbrokers in Georgian London



Clearly there is a substantial decrease in the prevalence of registered pawnbrokers in the records after 1815. While these records could be incomplete, Figure 1 provides anecdotal evidence consistent with economic theory and supports our identification strategy.

2.3 Old Bailey

Those accused of committing a crime in the city of London or Middlesex County would have their cases heard at the Old Bailey, which handled felony charges and more serious misdemeanors. Less serious offenses were taken to lower magisterial courts.

Since the 17th century, information on cases tried at the Old Bailey were commonly published. In 1679 the printing of accounts at the Old Bailey was incorporated into an annual publication known as the *Old Bailey Proceedings*. In 1787 the publication was subsidized and copies of the Proceedings were distributed to public officials. Its use as an official record of criminal trials in London continued until the early 1900s.⁸ Recently, the Proceedings have been digitized.

The Proceedings provide a unique opportunity to explore the testimony, questioning, and rulings at the Old Bailey. Entries include the identify of the judge and accused, provide a transcript of the testimony given at trial, and present the ruling and sentence (if applicable).

Data taken from the Old Bailey has served as a source for scholarship in the economics of legal institutions (Bindler and Hjalmarsson, 2018; McCannon and Porreca, 2021). The text of the trial transcripts has proven useful in appreciating life in London during this period. For example, Voth (1997) uses the testimony to measure labor supply during this time period. Bindler and Hjalmarsson (2021) evaluates the impact of the introduction of a professional police force by measuring the number of crimes going to the Old Bailey. Age

⁸See oldbaileyonline.org.

(Bindler and Hjalmarsson, 2017) and gender differences in prosecution (Bindler and Hjalmarsson, 2019) have also been evaluating by considering trials at the Old Bailey.

3 Old Bailey Results

With these trial transcripts, we execute our analysis with the following steps. First, we scrape all transcripts of trials at the Old Bailey between 1674 and 1913. There are 196,590 trials. Second, we limit our analysis to include 15 years between the Act and the licensing fees, and the 30 years after its introduction. Thus, we limit attention to the 76,956 trials at the Old Bailey between 1800 and 1845.

Next, we use a dictionary method identifying when a pawn shop is referenced within the trial’s text. We create a list of typical words, such as “pawn”, “pawnbroker”, “pledge”, and “redeem”,⁹ to record the mention of pawn shops. Overall, 14.09% of the observations mention a pawn shop in the testimonies.

Fourth, we identify which crimes are committed in each case. There are 53 unique crimes identified. We create an “unknown” category created for transcripts that fail to identify the crime. This makes up 0.83% of the observations. We also include an “other” category for unclassifiable crimes. This makes up 0.67% of the observations. Thus, the crime committed can be identified for most observations. Some crimes are rather frequent, such as grand larceny ($N = 26,880$), theft from a specified place ($N = 8277$), and pickpocketing ($N = 8714$).

From this, we identify which crimes involve a pawn shop in at least 10% of the cases. The collection of these crimes we label as *Theft*. This is, of course, a loose term as it encompasses ten different, but similar, crimes and allows for certain types of larcenies to not be included (e.g., animal theft). The crimes making up the theft category are listed in Table 1.

The table also includes the list of crimes that rarely involve pawn shops, as measured by the words used in the trial transcripts. For expositional ease, we will refer to this category of crimes as the “control” crimes, as it will make up the untreated crimes in the upcoming analysis. Specifically, these crimes rarely involving pawn shops have less than 2% of the observations referencing them in the trial’s text.

While thefts make up 20.8% of the crime types over the 1800-45 time period, they make up 69.3% of the case-level observations. Amongst the thefts, pawn shops/pawnbrokers are referenced in 18.5% of the observations.¹⁰

3.1 Time Series Analysis

With this data, we first collapse it to an annual time series. A number of annual proportional variables are created. With them, we estimate the following specification:

$$Y_y = \alpha_0 + \alpha_1 License_y + \epsilon_y. \tag{1}$$

⁹A total of 13 words, removing capitalization and end-of-sentence punctuation, are used to account for parts of speech and verb tense.

¹⁰For the control crimes pawn shops are referenced in the trial testimony in 1.7% of the observations, and arise in 6.7% of the case-level observations for the rest of the crimes (i.e., those not in the theft category or control group). They make up 10.0% and 20.8% of the observations, respectively.

Table 1: Crimes Involving Pawn Shops

crimes commonly involving pawn shops “thefts”	crimes rarely involving pawn shops “control”
conspiracy	animal theft
bankruptcy	assault
grand larceny	assault with intent
housebreaking	assault with sodomitical intent
receiving	barratry
seducing from allegiance	bigamy
simple larceny	coining offences
stealing from master	concealing a birth
theft from a specified place	embezzlement
treason	game law offences
shoplifting	illegal abortion
	infanticide
	keeping a brothel
	libel
	mail theft
	manslaughter
	petty larceny
	piracy
	rape
	religious offences
	returning from transportation
	seditious libel
	tax offences
	unknown

If 10% or more of a particular crime’s observations include at least one pawn-related word, then it is included in the list of thefts in the first column. If 2% or less of a particular crime’s observations include at least one pawn-related word, then it is included in the list of control crimes in the second column.

Table 2: Old Bailey and Pawn Shops

	Prop. of crimes that are thefts	Prop. of crimes involving pawn shops	Prop. of thefts involving pawn shops	Prop. of (non-pawn) crimes that are (non-pawn) thefts
After the licensing fees	-0.0493 *** (0.0121)	-0.0011 (0.0074)	0.0134 (0.0096)	-0.0594 *** (0.0128)
R^2	0.2748	0.0005	0.0419	0.3281
AIC	-167.4	-212.2	-188.1	-162.0

Annual values (as proportions of the totals) from 1800 to 1845 considered ($N = 46$). Omitted category is the period of pawn shop promotion (1800-14). Each specification includes an unreported constant. Standard errors reported in the parentheses; *** 1%, ** 5%, * 10% level of significance.

$License_t$ is equal to one if the observation arises in the years after the licensing fee's imposition on pawn shops; $y \geq 1815$. The omitted years are the years where pawn shops should be flourishing in London as they have been legitimized, allowed to charge a high interest rate, clarified borrowing terms, but do not yet have the high licensing fees (1800-14). A number of dependent variables will be considered. Table 2 presents the results.

The first column considers the proportion of crimes in a year that are thefts. The estimated coefficient is negative and highly statistically significant. This coincides with past research which suggests that pawn shops promote crime since the restriction in market size corresponds to fewer thefts.¹¹

Interestingly, the second column considers the proportion of crimes in a year (of any type) that involve pawn shops, using the published text of the trial transcripts. There is not a difference across the regulatory regimes. The involvement of pawn shops in any type of crime is smooth over time.

Adding to this unexpected outcome, the third column considers the proportion of thefts that involve pawn shops. Again, there is no statistically significant difference between the era of pawn shop promotion and pawn shops's prevalence in trial testimony. If pawn shops promote crime, then one would have expected nonzero coefficient in the second and third columns.

The fourth column provides an answer to this conundrum. It removes all cases that involve a pawn shop (both in thefts and non-theft crimes). The proportion of (non-pawn) crimes that are (non-pawn) thefts increase over this time period. Hence, the observation in the first column – that thefts increases once the pawnbroking industry is legitimized, but before licensing fees drive many of them out of business – is coming from an increase in thefts that are unrelated to pawn shops. This supports the argument that pawn shops are only a minor outlet for stolen goods and that their closure does not mitigate the prevalence of total property crime. Figure 2 provides graphical depictions of thefts over time in Georgian London.

¹¹The total number of thefts increase after 1815, but the total number of all crimes increases by even more. If the first column is re-estimated, but with the total number of thefts as the dependent variable, the coefficient on the time break indicator is 800.07 with $p < 0.001$.

Figure 2: Crime in Georgian London



The figure considers the number of thefts, as a proportion of all crimes at the Old Bailey, over time. The dashed line subtracts those thefts that reference pawn shops. The reduction is small and the gap between the two lines is rather stable. The policy change in 1815 creates no noticeable change in the time trend. The figure coincides with the regression results in Table 2.

3.2 Panel Data Set

We can take this analysis further. Rather than consider thefts as a proportion of total crimes in a year, we count the number of thefts arising each year over the 1800-45 time period. Further, we count the number of control crimes (i.e., those crimes that rarely involve a reference to a pawn shop). With these, we build a panel data set of $N = 92$ observations (2 crime categories x 46 years). We estimate the following difference-in-difference specification:

$$Crime_{ty} = \beta_0 Treated_t + \beta_1 License_y \times Treated_t + \tau_y + \epsilon_{ty}. \quad (2)$$

$Crime_{ty}$ is the number of crimes of type t (either thefts or control crimes) occurring in year y . The indicator variable $Treated_t$ is equal to one for the thefts, i.e., the treated crime type, and is equal to zero for the control crime category. The indicator variable $License_y$ is equal to one for years after the licensing fee was implemented ($y \geq 1815$). Indicator variables for each year, τ_y , are included as well. The coefficient β_1 is the difference-in-difference coefficient of interest. If $\hat{\beta}_1 < 0$, then the reduction in the number of pawn shops operating in London leads to less crime. Table 3 presents the estimation results.

Table 3: Prevalence of Theft in Georgian London

Dep. var. =	Crime (total) [1]	Crime (detrended) [2]
Treated	538.00 *** (69.94)	-63.32 ** (29.45)
After the licensing fees x Treated	673.16 *** (85.19)	0.483 *** (0.029)
Year Fixed Effects?	Yes	Yes
R^2	0.953	0.965
AIC	1256.1	1108.6
DV μ	663.09	217.63

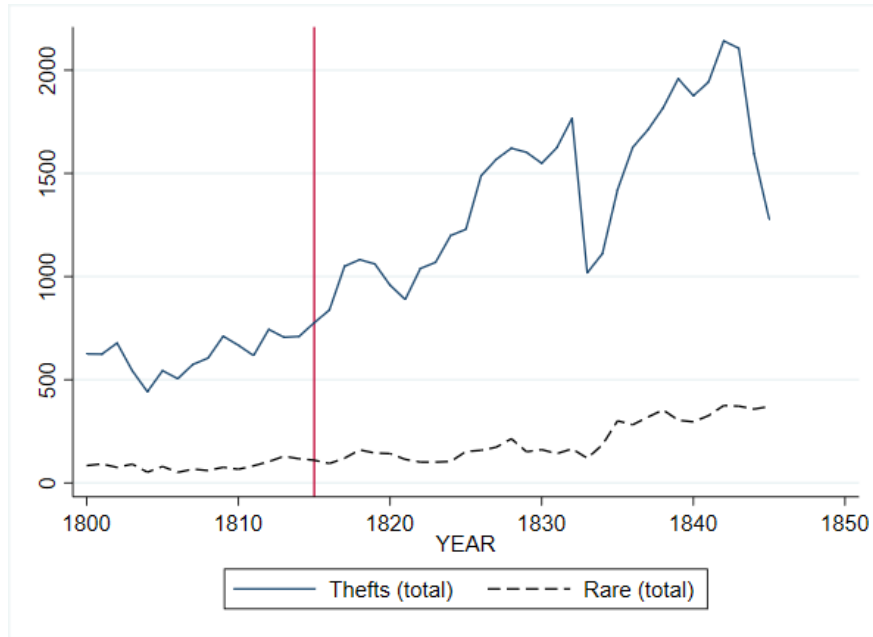
Dependent variable is the number of crimes of that type (either theft or rare) for the year. The first column uses the total value for the year. The second column detrends the dependent variable by estimating a linear time trend in the pre-treatment periods for each crime type separately. Annual values from 1800 to 1845 considered; $N = 92$. Standard errors reported in the parentheses; *** 1%, ** 5%, * 10% level of significance.

Column [1] provides an intriguing result. Using the thefts as the treated crime type, the difference-in-difference coefficient is positive and statistically significant. This suggests that the reduction in the number of pawn shops *increases* the number of thefts, contrary to the dominant narrative.

This result can only be taken as causal if: (i) the policy does not affect the control crimes and (ii) the treated and control crime types are following parallel trends in the years prior to the policy change. For the first requirement (i.e., SUTVA), suggestive evidence exists that the control crimes are unaffected by the licensing fee for pawn shops. First, the category was constructed to be those crimes unrelated to pawn shops and, hence, will be unrelated to changes in pawn shop regulation. Second, using the case-level data set over 1800 to 1845, the proportion of cases that reference a pawn shop is 1.71% prior to 1815, and is 1.70% after. A difference-in-means t -test is highly insignificant ($t = 0.03$; $p = 0.98$). Thus, it is reasonable to presume that imposing the licensing does not affect our control group.

Regarding (ii), evidence exists that thefts and the control crimes are *not* following parallel trends. Figure 3 provides a graphical representation of the time trends of thefts and control crimes over time.

Figure 3: Crime in Georgian London



The solid line depicts the total number of thefts for each year from 1800 to 1845. The dashed line depicts the total number of control crimes each year.

Clearly, while the number of control crimes is flat over the time period, thefts are trending upward. This divergence in the time trend not only occurs after the policy is implemented, but begins prior to it. For simple, econometric evidence of this second point, we estimate a simple linear time trend model over the 1800-14 time period with both a linear time trend and the linear time trend interacted with the treatment indicator (and a constant term). The estimated coefficient on the linear time trend (consequently, the trend for the control crimes) is 5.962 ($p < 0.05$) and the estimated coefficient on the interaction term (hence, the divergence between the two time trends in the pre-licensing fee era) is 0.298 ($p < 0.001$). Thus, the two are following different time trends prior to the policy’s implementation. This is a concern. Since thefts are increasing at a more rapid pace prior to the introduction of the large licensing fee, then the divergence measured in column [1] of Table 3 may not be the causal effect of the policy change on crime, but instead may be an artifact of the underlying time trends.

To account for the non-parallel trends, we detrend the observations. Specifically, for the 1800-14 time period we run a linear regression with the number of crimes as the dependent variable and a linear time trend as the independent variable. We do this for both the thefts and the control crimes independently. With the estimated intercept and slope terms, we calculated the fitted value of the crimes and subtract this from the actual number of times for the entire time period (1800-45). Thus, the new, detrended, dependent variable is the residual. While the values for the detrended crimes will have a zero mean for both the thefts and the “rare” crimes prior to the policy change, the values after the licensing fee is implemented accounts for breaks from the pre-treatment trends. Further, the pre-treatment time trends are now parallel. Pooling

the two crime types, and using the detrended crime variable as the dependent variable, an interaction term between the treatment indicator and the year time trend is highly statistically insignificant and essentially zero if estimated on the 1800-14 time period (rather, it has a coefficient of -4.1×10^{-6}).¹² Consequently, this detrending process maintains compliance with the SUTVA requirement and emits a data set where the values are following parallel trends prior to the treatment.

Column [2] of Table 3 provides the result. The difference-in-difference coefficient remains positive and statistically significant. Thus, the results from our analysis of trials at the Old Bailey suggest that the reduction in the number of pawn shops caused an increase in thefts.

4 Pawnbroking in the United States

The literature on modern pawn shops in America is surprisingly sparse, given that it provides financing to a marginalized group of people who are unable to access credit any other way. Caskey (1991) provides an early descriptive analysis of pawnbroking across the U.S., finding that customers who turn to pawn shops for credit do so because their incomes or credit histories make them ineligible for bank and other finance company loans. The benefits of this form of financing is often misunderstood, as both Carter (2015) and Davies and Hall (2018) find credit financing available at pawn shops offers a more secure alternative to payday loans, and specifically, the activity of rolling over payday loans, which is ultimately more expensive for borrowers.

As an alternative secure lending institution, borrowing behavior, repayment rates, and formal banking policies are also important concerns of pawnbrokers. For example, Carter and Skiba (2012) use a unique data set of transactions from 103 pawn shops across Texas to study pawn shop borrowing behavior. Looking at collateralized pledges across borrowers, they find that borrowers are more likely to pay back loans when they have pawned a sentimental item, relative to the dollar value of the item. When it comes to the effects of policy on credit in pawn shops, Shackman and Tenney (2006) discusses how policies regarding interest rate ceilings and requirements to return excess proceeds from the sale of collateral items negatively affects alternative lending institutions. Similarly, McKernan et al. (2013) find that 3% interest rate cap is associated with a 23% reduction in pawn shop borrowing.

Even though pawn shops provide credit to an otherwise marginalized population, pawn shops are often put in a category of locations known as “crime generators”, which include places like bars and liquor stores. We ask whether this label as a crime generator is justified.

Where pawnbrokers choose to locate is an important part of understanding their impact in communities. Sawyer (2004) and Prager (2009) study the locations of pawn shops and find that their locations are disproportionately located in minority and low-income neighborhoods. Other determinants include measures of the population’s credit worthiness and the stringency of state laws and regulations governing alternative financing institutions. Further, Miller et al. (2018) explore geographical differences in the readability

¹²This specification includes a constant, treated crime indicator, and a linear time trend as well. Estimating this over the 1815-45 time period, the interaction term is positive, large, and highly statistically significant (16.53; $p < 0.001$).

of pawnbroker regulations across the U.S., as measured by the average number of words per sentence and average syllables per word. While they find regional differences in the readability of regulations, the location of the pawnbrokers is not correlated with the readability of pawnbroker regulations in a given state.

As mentioned previously, the studies by Kubrin and Hipp (2016) and d’Este (2020) relate directly to our investigation. In the former, Kubrin and Hipp (2016) consider the cross-sectional variation in crime across Los Angeles and show that it correlates with closeness to alternative financial institutions (most notably pawn shops). They recognize that their results cannot be interpreted as being causal as they are unable to account for pawnbroker’s location decision. d’Este (2020), as stated, considers a panel data set and also shows a positive relationship between the number of active pawn shops and property crime. To make a causal claim, in a robustness analysis he considers differential effects of changes in gold prices on burglaries. Those counties with a higher concentration of pawn shops experience disproportionate increases in burglaries when gold prices increase. The important omitted factor in these studies is that the number of pawn shops operating in an area is endogenous. There are likely important social and economic factors that drive the locational decisions of pawn shops, and there is good reason to believe that these same unmeasured factors drive crime.

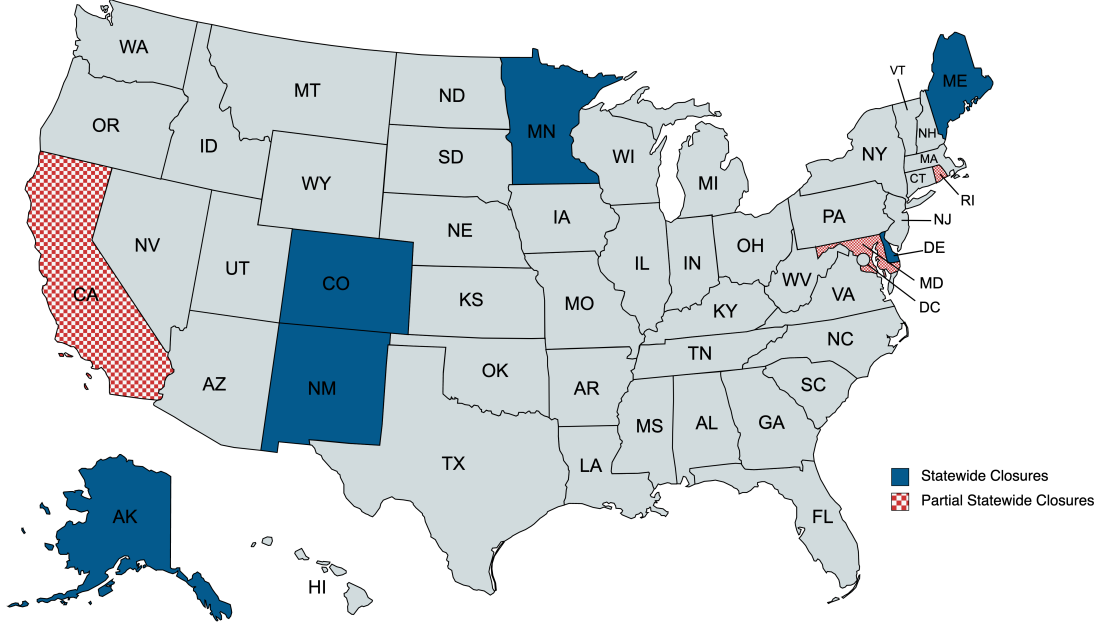
As shown in our analysis of the Old Bailey, evaluating the correlation between the number of pawn shops and total property crime potentially misses the prevalence of crimes that are specifically induced by access to pawn shops as non-pawn-related crime dwarfs pawn-related crime. In the next section, we take advantage of variation in closure policies, which directly affect access to pawn shops to identify their causal impact on property crime.¹³

5 Coronavirus Lockdowns

The Covid-19 pandemic has had a tremendous impact on the world. The virus reached the United States in early February 2020 and by the end of March 2020 policy responses were implemented. Unlike many countries with a centralized, national response, state governments in the U.S. led. Consequently, there was variation in lockdown policies. One important dimension was the classification of pawn shops as an “essential business”. Many states regulate pawn shops as an enterprise in the financial industry. Consequently, as banks were deemed to be essential, so too were pawn shops. A handful of states, though, treated pawn shops distinctly from other financial institutions and shut them down.

¹³As a final example of research proposing a positive relationship between pawn shops and crime, Fass and Francis (2004) presents survey data from Texas showing that users of pawn shops also have a history of criminal arrest. Of course, this does not necessarily mean that they are using pawn shops for stolen goods as those with arrest records could have lower average human capital and the arrest record itself could be making employment more difficult. Thus, this can simply be a correlation caused, ultimately, by financial distress.

Figure 4: Statewide Pawn Shop Closures During Covid-19 Lockdowns



Six states closed their pawn shops (depicted in Figure 4).¹⁴ Hence, there was variation across the country in whether pawn shops were open in April, May, and June of 2020. We exploit this variation in a difference-in-difference specification to identify the causal impact of pawn shop access on property crime. Specifically, we estimate:

$$Crime_{smy} = \gamma_1 TreatedPeriod_{my} \times LockdownState_s + \sigma_s + \mu_{my} + \epsilon_{smy}, \quad (3)$$

where $Crime_{smy}$ is the number of crimes in state s in month m of year y . Our primary focus will be on property crimes. State fixed effects, σ_s , will be included to control for time invariant differences across the country. Month by year fixed effects, μ_{my} , are included to capture temporal variation. The coefficient γ_1 is the difference-in-difference coefficient of interest. If $\hat{\gamma}_1 < 0$, for example, then the lockdown of pawn shops in the state corresponds to less crime, which would support the mainstream view that pawn shops facilitate it.

Therefore, our difference-in-difference estimation strategy asks whether crime in the six states who closed pawn shops diverges from crime in those who did not during April, May, and June of 2020. We use monthly reported crime data provided by the FBI in their Uniform Crime Report.¹⁵ The reported crimes from each law enforcement agency is aggregated to the state level for each month between January 2009 and December

¹⁴Another three states partially shut down pawn shops. This includes closing operations for everything but the loan services (Rhode Island), city-level shutdowns in specific areas of the state (California), and only very short-term closing relaxed by the first week of May (Maryland).

¹⁵Specifically, we use (and appreciate) the data organized by Kaplan (2021).

Table 4: Covid-Related Pawn Shop Shutdowns

	Property [1]	Larceny [2]	Burglary [3]	Murder [4]
Treated Period x Lockdown State	4351.72 *** (1195.93)	2836.06 *** (862.21)	1220.26 *** (267.01)	-1.80 (3.43)
State Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.406	0.376	0.388	0.424
AIC	162,683.8	157,864.6	140,962.4	72,204.9
DV μ	13107.34	9129.11	2703.76	25.74

The variable *Treated Period* is equal to one if and only if the observation occurs in April, May, or June of 2020. The variable *Lockdown State* is equal to one if and only if the observation occurs in one of the six states that shut down the pawn shops. Each specification differs in which crime is used as the dependent variable. Standard errors are clustered at the state by year level (600 clusters); *** 1%, ** 5%, * 10% level of significance.

2020. Hence, $N = 7200$. Table 4 presents the estimation results. Standard errors are clustered at the state level.

Contrary to common opinion, the results in Table 4 indicate that the closing of pawn shops increased property crime; column [1]. The estimated effect is approximately 4350 additional property crimes in each of the lockdown states per month. This corresponds to approximately a $\frac{1}{6}^{th}$ of a standard deviation increase. Thus, it is an economically significant effect as well.

Columns [2] and [3] consider two important subcategories of property crimes. Both experiences statistically significant increases. Column [4] considers murder. This serves as a falsification test. It is unlikely that pawn shop closures affect murder rates, and both Kubrin and Hipp (2016) and d’Este (2020) also use it as a check on the validity of result. Unsurprisingly, we find that pawn shop openings are unrelated to the murder rate.

Finally, if the three partially closed states are added to the treatment effect, the difference-in-difference coefficient remains relatively large ($\hat{\gamma}_1 = 2861.87$) and retains its statistical significance ($p = 0.06$). The fact that the estimated effect shrinks and some of the significance is lost suggests that the effect of the restrictions is felt primarily when the pawn shops are fully shut down.

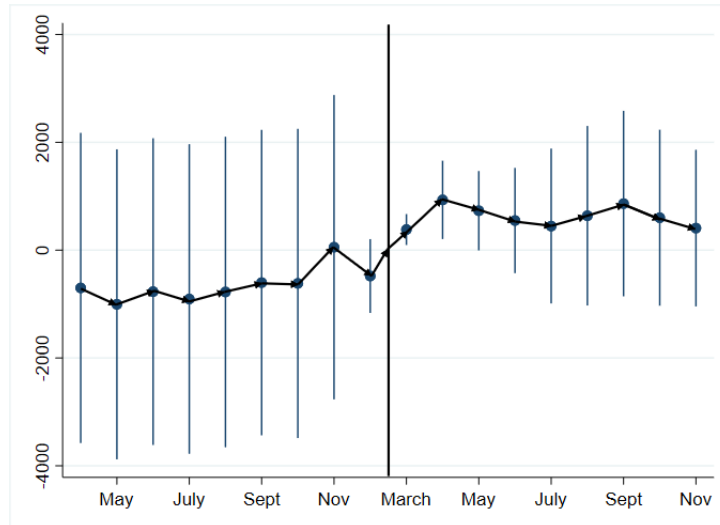
The primary result in column [1] can be interpreted as causal only if the lockdown states and the non-lockdown states were experiencing parallel trends prior to the pandemic. To assess this, we consider an event study. We focus on the twelve months prior to the pandemic through the end of 2020. An indicator variable is created for each month and interacted with the *Lockdown State* indicator variable. For time period t , this indicator variable is denoted $Shutdown_{smy}^t$. For the April 2019 to December 2020 time period, we estimate

$$Crime_{st} = \sum_{t=-12}^9 \delta_t Shutdown_{st}^t + \sigma_s + \mu_{my} + \epsilon_{smy}. \quad (4)$$

Hence, as stated, $Shutdown_{smy}^t$ is equal to one if and only if both the observation occurs in a state which

implemented the lockdowns and the observation occurs in period t . Time period $t = 0$ is March 2020. The time period just prior to the Covid-19 pandemic (February 2020) is the omitted time period. We use monthly, state-level property crime as the dependent variable. Figure 5 depicts the coefficient estimates and the 95% confidence intervals.

Figure 5: Covid Event Study



The point estimate for December 2019 and December 2020 are included in the estimation but not presented. They each have substantially larger confidence intervals than the rest. Hence, they are removed from the figure for readability. Both include zero in the 95% confidence interval. The regression also includes the full set of state fixed effects and month x year fixed effects.

As with the difference-in-difference specification, the property crime values lie above zero for April, May, and June of 2020. Importantly, each of the pre-pandemic confidence intervals include zero. Thus, there does not seem to be important discrepancies in the time trends prior to the pandemic. Hence, we can be confident that our estimates are representing a causal effect of the pawn shop closures.

We conduct a number of robustness checks to assess the sensitivity of our main result. First, we conduct a permutation test falsely assigning the treated locations and times.¹⁶ Only 5.9% of the “false” difference-in-difference coefficients are larger (in absolute value) than our true estimate presented in column [1] of Table 4.¹⁷ Using the distribution of t -statistics to create a two-tailed, p -value, our true difference-in-difference coefficient remains significant at the 5% level (i.e., Fisher exact p -value = 0.024). Second, given the relatively small number of cross-sectional units (i.e., the 50 states) and small number of treated cross-sectional units (6), one can be concerned that the results of one particular state can be having an oversized effect on the result. To address this, we engage in a leave-out process. We re-estimate our main result (column [1] of Table 4) dropping one state. Hence, it is estimated 50 times. In each of these regressions, the difference-in-difference coefficient is statistically significant at the 1% level. Further, the estimated effect is also quite stable. Overall, 37 of the 50 estimations generates a coefficient that is within 100 of the value derived from

¹⁶We permute the difference-in-difference term maintaining the proportion of observations with this interaction equal to one at the “true” level.

¹⁷Only 3.6% exhibit a positive effect larger than our result.

the full sample.¹⁸ Third, we consider alternative measurements of the dependent variable. For example, if the number of crimes is normalized by the state's population, the results are unaffected. Property crime per 100,000 residents increases by 42.0 (with $p = 0.011$). Log transforming the dependent variable gives an estimated difference-in-difference coefficient of 0.198 ($p = 0.046$).¹⁹ Also, the results presented in Table 4 consider the three month window. If it is expanded or contracted, as evidenced in the event study, the difference-in-difference coefficient shrinks in value (but remains significant if April is removed or if May (or May and June) are added as treated periods. Thus, our result is not sensitive to any one particular state, either a control or treated state, having property crime out of line with the rest of the country during this time period, in how the dependent variable is measured, or in the definition of the treated period.

The primary concern is that the lockdown of pawn shops is correlated with broader policies that restrict the state economy. Hence, financial distress may be actually causing the property crime, and pawn shop closures are correlated with financial distress but not causing crime. To address this concern we use monthly unemployment compensation claims. This is our measurement of financial distress. We ask whether unemployment can explain away our finding. Table 5 presents the results.

¹⁸In addition, 49 of the 50 are within 400. The exception is Florida. When it is excluded the difference-in-difference coefficient is 3377.79 with $p < 0.001$.

¹⁹This last result excludes Florida. In the data set commonly used (Kaplan, 2021), crime reporting in Florida is especially weak. The average number of property crimes per capita is reported to be 254.86 when the mean for the rest of the country is 225.49. Florida has, though, 66 monthly observations of 0 property crimes, but 13 observations over 1000. Re-estimating [1] excluding Florida results in a difference-in-difference coefficient of 3377.79 ($p < 0.001$). Including Florida in the log transformation (and setting a zero value equal to 0.001) results in an estimate of 0.128

Table 5: Unemployment Compensation Claims

	[1]	[2]
Treated Period x Lockdown State	1478.82 *** *** (735.91)	1460.83 (896.64)
Initial Claims	-0.019 *** (0.006)	-0.024 *** (0.007)
Initial Claims x Treated Period		0.007 ** (0.004)
Initial Claims x Lockdown State		-0.023 (0.032)
Initial Claims x Treated Period x Lockdown State		0.025 (0.032)
State Fixed Effects?	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes
R^2	0.408	0.408
AIC	162,668.6	162,673.8

The variable *Treated Period* is equal to one if and only if the observation occurs in April, May, or June of 2020. The variable *Lockdown State* is equal to one if and only if the observation occurs in one of the eight states that shut down the pawn shops. Each specification differs in the dependent variable used. Standard errors are clustered at the state by year level (600 clusters); *** 1%, ** 5%, * 10% level of significance. Monthly observations between 2009 and 2020 used; $N = 8350$.

Column [1] considers the primary specification in Table 4, but adds the initial unemployment claims as a control variable. While the added control is statistically significant, the main result is unaffected. Controlling for unemployment, states that shut down pawn shops experienced a divergence in the number of property crimes.

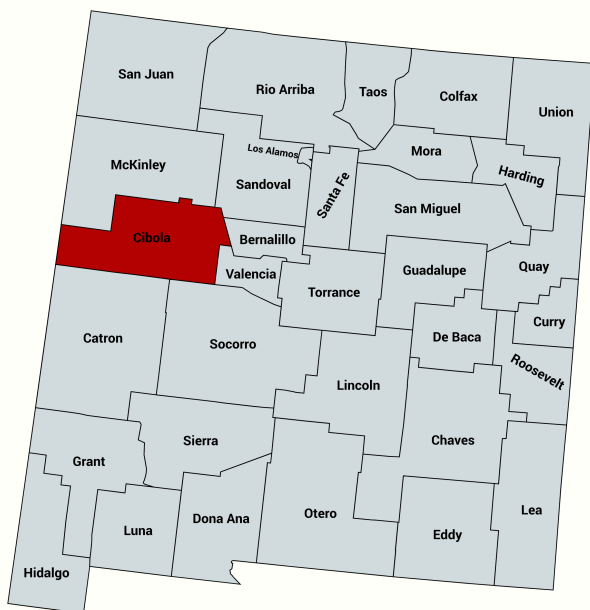
Column [2] estimates a triple difference specification adding interactions to the treated time period and lockdown state indicators, along with introducing the three-way interaction. Importantly here, the triple difference coefficient is statistically insignificant. The relationship between a state's pawn shop policies and the Covid-19 pandemic is not affected by the volume of unemployment.

6 New Mexico Case Study

A further test of the benefits derived from pawn shops can be found in the state of New Mexico. As shown in the previous section, state officials ordered the shutting down of all pawn shops. In one county, local officials did not comply. In the city of Grants, located in Cibola County (approximately 80 miles west of Albuquerque), the local mayor (and consequently city police department) refused to enforce the Governor's lockdown order. Given its small population, Mayor Martin Hicks felt that the city would be hurt more by closing businesses than by simply implementing social distancing policies (Gerstein, 2020). In defiance of

the order, he encouraged city employees to return to their offices and local businesses to reopen. This led to a rather public stand-off between the levels of government that garnered media attention (Block, 2020; Edwards, 2020; Klemko and Witte, 2020). Considerable, daily fines were levied against the open pawn shops by the state government²⁰

Figure 6: Cibola County Defied State Covid-19 Lockdown Order



The academic value of this situation is that it creates within-state variation in the availability of pawn shops. While people across the state were subject to the same economic, political, and health shocks, one area refused to implement the policy of interest. Therefore, we construct a panel data set aggregating the number of property crimes at only the county level. There are 33 counties in New Mexico. We eliminate Bernalillo County (i.e., Albuquerque) because its population and, consequently, crime, is substantially larger than any other county in the state. For the 32 counties considered, we measure the number of property crimes per month during 2019 and 2020 (i.e., 24 months). We estimate the following difference-in-difference specification:

$$Property_{cmy} = \theta_1 TreatedPeriod_{my} \times Cibola_c + \kappa_c + \mu_{my} + \epsilon_{cmy}. \quad (5)$$

Both county fixed effects, κ_c , and month by year fixed effects, μ_{my} , are included. As before, keeping open the pawn shops in Cibola County helped to relieve financial distress which acts to dissuade property crime when $\hat{\theta}_1 < 0$. Standard errors are clustered at the county level. Table 6 presents the results.

²⁰A check on pawnguru.com indicates that there are 25 pawn shops in Grants, NM (as of December 2021).

Table 6: Case Study: Cibola County, New Mexico

	[1]	[2]	[3]
Treated Period x Cibola County	-7.92 ** (3.62)	-5.95 * (3.07)	-7.37 ** (3.24)
County Fixed Effects?	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes
Added Treatment Interaction	none	3 Months Prior	3 Months After
R^2	0.837	0.837	0.837
AIC	7906	7906	7906

Dependent variable is the number of property crimes reported per month (during 2019 and 2020) for each county (excluding Bernalillo County); $N = 768$. The variable *Treated Period* is equal to one if and only if the observation occurs in April, May, or June of 2020. The variable *Cibola County* is equal to one if and only if the observation occurs in that county. Each specification differs in whether any other interaction term is included. Column (2) adds an interaction between Cibola County and an indicator for the three months prior to the treatment period (January through March 2020), while column (3) adds an interaction between Cibola County and an indicator for the three months after the treatment (July through September 2020). Standard errors are clustered at the county level (32 clusters); *** 1%, ** 5%, * 10% level of significance.

Once again, keeping open pawn shops is associated with reductions in property crime during the Covid-19 pandemic. There were approximately 6 to 8 fewer property crimes in Cibola County each month in spring 2020. This is economically significant as the county of 26,675 averaged 27.1 property crimes per month prior to the pandemic. Looking at data within New Mexico provides important support as other pandemic-related policies are applied to both the treated county (Cibola) and the untreated ones (the other 31 counties in the state). Columns [2] and [3] differ from the baseline specification in [1] in the inclusion of either a lead or lagged effect. Both, when included, are statistically insignificant at the 10% level. Thus, diverging pre-trends do not seem to be driving the result, [2], and the effect dissipates once lockdown policies begin to ease, [3].

7 Conclusion

A narrative that pawn shops provide a positive externality in property crime reduction more appropriately conforms to the empirical reality. Exploring both the reduction in pawn shops in Georgian London and the state-level variation in the essential-business classification of pawn shops in Covid-era United States, we provide evidence that the closure of pawn shops does not necessarily reduce property crime, but may instead increase it.

Each data set has its limitations worth pointing out. The identification strategy used in the data from London uses transcripts from trials at the Old Bailey. We are unable to account for minor offenses going to lower-level courts, the decision to not prosecute a case, or the effectiveness at identifying and arresting the accused. While this means we are under-reporting the number of crimes, the validity of our results would only be questioned if the un-measured crime correlates with our treatment variable. We have no reason to believe, for example, that the decision to prosecute a case correlates with pawn shop licensing fees. For the Covid-19 data, one wonders whether other pandemic-driven policies could be coupled with the classification

of pawn shops is a concern. The case study within New Mexico mitigates this concern. Finally, external validity can be questioned. The Coronavirus pandemic is unique for the modern (at least Western) world. We cannot say that behavior at this time, and specifically policy-induced behavioral changes, would necessarily extrapolate to “normal” times. Notwithstanding these concerns, our documentation that pawn shops do not promote crime using data in different countries in time periods far apart reinforces our results.

Moving forward, policymakers concerned with property crime may find more fruitful efforts than regulation and restricting pawning activities. Pawnbrokers provide a valuable service to their consumers, and we provide evidence that this likely reduces pressures to commit crime. Individuals who sell or pawn items with pawnbrokers must create a record of the transaction, which is made available to law enforcement. Alternatives, such as flea markets, informal trade, and virtual markets can be expected to be a safer avenue to sell stolen items. Therefore, the prevalence of pawn shops is unlikely to be a catalyst of crime.

References

- Bindler, A. and R. Hjalmarsson (2017). Prisons, recidivism, and the age-crime profile. *Economics Letters* 152, 46–49.
- Bindler, A. and R. Hjalmarsson (2018). How punishment severity affects jury verdicts: Evidence from two natural experiments. *American Economic Journal: Economic Policy* 10(4), 36–78.
- Bindler, A. and R. Hjalmarsson (2019). The persistence of criminal justice gender gap: Evidence from 200 years of judicial decisions. *Journal of Law and Economics* 63(2), 297–339.
- Bindler, A. and R. Hjalmarsson (2021). The impact of the first professional police forces on crime. *Journal of the European Economic Association*.
- Block, J. (2020). Mlg punishes cibola county after grants mayor disobeys: Pawnshop fined \$60k, county shut down. *Pinon Post*, (May 1).
- Bolen, J. B., G. Elliehausen, and T. W. Miller Jr. (2020). Do consumers need more protection from small-dollar lenders? historical evidence and a roadmap for future research. *Economic Inquiry* 58(4), 1577–1613.
- Brealey, P. (2013). The charitable corporation for the relief of industrious poor: Philanthropy, profit and sleaze in london, 1707–1733. *History* 98(333), 708–729.
- Carr, C. T. (1913). *Select Charters of Trading Companies, A.D. 1530-1707*. B. Quaritch.
- Carrell, S. and J. Zinman (2014). In harm’s way? payday loan access and military personnel performance. *Review of Financial Studies* 27(9), 2805–2840.
- Carter, S. P. (2015). Payday loan and pawnshop usage: The impact of allowing payday loan rollovers. *Journal of Consumer Affairs* 49(2), 436–456.
- Carter, S. P. and P. M. Skiba (2012). Pawnshops, behavioral economics, and self-regulation. *Review of Banking and Financial Law* 32, 193.
- Carter, S. P. and W. Skimmyhorn (2014). Much ado about nothing? new evidence on the effects of payday lending on military members. *Review of Economics and Statistics* 99(4), 606–621.
- Caskey, J. P. (1991). Pawnbroking in america: The economics of a forgotten credit market. *Journal of Money, Credit and Banking* 23(1), 85–99.
- Coffey, T. (1966). Beer street: Gin lane. some views of 18th-century drinking. *Quarterly Journal of Studies on Alcohol* 27(4), 669–692.
- Cuffe, H. E. and C. G. Gibbs (2017). The effect of payday lending restrictions on liquor sales. *Journal of Banking and Finance* 85, 132–145.

- Davies, E. and J. C. Hall (2018). The unintended consequences of consumer finance regulations. *Unilag Law Review* 2(2), 31–57.
- De Roover, R. (1946). The three golden balls of the pawnbrokers. *Business History Review* 20(4), 117–124.
- Dobbie, W. and P. M. Skiba (2013). Information asymmetries in consumer credit markets. *American Economic Journal: Applied Economics* 5(4), 256–282.
- d’Este, R. (2020). The effects of stolen-goods markets on crime: Pawnshops, property theft, and the gold rush of the 2000s. *Journal of Law and Economics* 63(3), 449–472.
- Edwards, C. (2020). Nm gun shop facing \$60k fines for remaining open. *Bearing Arms*, (May 2).
- Engle, S., J. Stromme, and A. Zhou (2020). Staying at home: Mobility effects of covid-19. *Working Paper*.
- Fass, S. M. and J. Francis (2004). Where have all the hot goods gone? the role of pawnshops. *Journal of Research in Crime and Delinquency* 41(2), 156–179.
- Gerstein, M. (2020). Grants pawn shop fined \$60k for defying public health order. *Santa Fe New Mexican*, (May 1).
- Hall, J. C. and B. C. McCannon (2021). Stay-at-home orders were issued earlier in economically unfree states. *Southern Economic Journal* 87(4), 1138–1151.
- Kaplan, J. (2021). Jacob kaplan’s concatenated files: Uniform crime reporting program data: Offenses known and clearances by arrest (return a), 1960-2020. *Consortium for Political and Social Research [distributor]*.
- Klemko, R. and G. Witte (2020). America’s coronavirus divide is reflected in two new mexico mayors. one asked for a lockdown. the other defied orders. *Washington Post*, (May 7).
- Kubrin, C. E. and J. R. Hipp (2016). Do fringe banks create fringe neighborhoods? examining the spatial relationship between fringe banking and neighborhood crime rates. *Justice Quarterly* 33(5), 755–784.
- McCannon, B. C. (2021). Do governors lead or follow? timing of stay-at-home orders. *Eastern Economic Journal* 47(4), 506–518.
- McCannon, B. C. and Z. Porreca (2021). The right to counsel: Criminal prosecution in 19th century london. *Working Paper*.
- McKernan, S.-M., C. Ratcliffe, and D. Kuehn (2013). Prohibitions, price caps, and disclosures: A look at state policies and alternative financial product use. *Journal of Economic Behavior & Organization* 95, 207–223.
- Melzer, B. T. (2011). The real costs of credit access: Evidence from payday lending market. *Quarterly Journal of Economics* 126(1), 517–555.

- Miller, G., S. A. Hanke, and H. Di (2018). Pawnshops regulatory environment: A readability analysis. *Journal of Accounting, Business and Management* 25(1), 50–61.
- Minkes, A. (1953). The decline of pawnbroking. *Economica* 20(77), 10–23.
- Morgan, D. P., M. R. Strain, and I. Seblani (2012). How payday credit access affects overdrafts and other outcomes. *Journal of Money, Credit, and Banking* 44(2-3), 519–531.
- Morse, A. (2011). Payday lenders: Heroes of villains? *Journal of Financial Economics* 102(4), 28–44.
- Nevill, S. (1732). *The Historical Register Volume XVII. For the Year 1732*. Nevill, Nutt, Gosling, Browne, and King.
- Penderel-Brodhurst, J. G. J. (1911). *The Encyclopedia Britannica: 11th ed.* Encyclopedia Britannica, Inc.
- Prager, R. A. (2009). Determinants of the locations of payday lenders, pawnshops and check-cashing outlets. *Review of Industrial Organization* 45(1), 21–38.
- Raymond, R. J. (1978). Pawnbrokers and pawnbroking in dublin: 1830 - 1870. *Dublin Historical Record* 32(1), 15–26.
- Sawyer, N. (2004). *Analysis of Alternative Financial Service Providers*. Washington D.C.: The Urban Institute.
- Shackman, J. D. and G. Tenney (2006). The effects of government regulations on the supply of pawn loans: Evidence from 51 jurisdictions in the u.s. *Journal of Financial Services Research* 30(1), 69–91.
- Skiba, P. M. and J. Tobacman (2019). Do payday loans cause bankruptcy? *Journal of Law and Economics* 62(3), 485–519.
- Stegman, M. A. (2007). Payday lending: A business model that encourages chronic borrowing. *Journal of Economic Perspectives* 21(1), 169–190.
- Sumption, J. (1999). *The Hundred Years War*. University of Pennsylvania Press.
- Swain, W. and K. Fairweather (2012). The legal regulation of pawnbroking in england, a brief history. In J. Devenny and M. Kenney (Eds.), *Consumer Credit, Debt, and Investment in Europe*, pp. 142–159. Cambridge University Press.
- Voth, H.-J. (1997). Time and work in eighteenth-century london. *Journal of Economic History* 58(1), 29–58.

8 Supplemental Appendix (not for publication)

8.1 Additional Results Regarding the Old Bailey Analysis

First, Table 7 lists those words used to identify the presence of pawn shops in the Old Bailey trial testimony.

Table 7: Dictionary of words used to Identify Pawn-Related Cases

Dictionary Terms
pawn
pawn'd
pawned
pawnbroker
pawnbroker's
pledged
pledge
redeem
pawn-broker
pawn-broker's
pawnbrokers
pawn-brokers
pawnd

We list all words used to construct our pawn shop dictionary. All capitalization and end-of-sentence punctuation was first removed.

Next, Table 8 lists those crimes included in the theft category. It presents the total number of observations, decomposing it into the crimes during 1800-14 timer period and those in the 1815-45 time period. The final column provides the proportion of these observations (over the 1800-45 time period) that reference a pawn shop.

Table 8: Treated “Thefts”

crime	# obs.	1800-14	1815-45	prop. with pawn references
conspiracy	125	23	102	0.120
bankrupcy	10	5	5	0.200
grand larceny	16,588	6752	9836	0.153
housebreaking	1576	206	1370	0.180
receiving	2422	390	2032	0.250
simple larceny	19,858	0	19,858	0.191
stealing from master	5125	0	5125	0.195
theft from a specified place	6348	1467	4881	0.233
treason	3	0	3	0.667
shoplifting	1254	452	802	0.109

If 10% or more of a particular crime’s observations include at least one pawn-related word, then it is included in the list of thefts in the first column. If 2% or less of a particular crime’s observations include at least one pawn-related word, then it is included in the list of control crimes in the second column.

As illustrated in Table 8, three crimes did not experience any observations prior to 1815. This can be due to changes in the naming of the illegal activity, or a change in venue for the prosecution of the act.

Regardless, we want to ensure that the change in the volume of crime is not affected by the addition of new activities, but reflects strictly the change in criminal activity. Hence, the time series results are re-estimated, but with all observations involving these three crimes eliminated from the data set (prior to it being collapsed to the annual level). Table 9 presents the results.

Table 9: Time Series Results: Dropping 3 Thefts

	Prop. of crimes that are thefts	Prop. of crimes involving pawn shops	Prop. of thefts involving pawn shops	Prop. of (non-pawn) crimes that are (non-pawn) thefts
After the licensing fee	-0.2021 *** (0.0327)	-0.0224 *** (0.0070)	0.0170 (0.0113)	-0.2154 *** (0.0345)
R^2	0.464	0.191	0.049	0.469
AIC	-75.6	-218.0	-173.7	-70.7

Annual counts (as proportions of the totals) from 1800 to 1845 considered ($N = 46$). Omitted category is the period of pawn shop promotion (1800-14). Each specification includes an unreported constant. Standard errors reported in parentheses; *** 1%, ** 5%, * 10% level of significance.

Further, the time series analysis of the Old Bailey data considers only dependent variables that are measured as proportions. Table 10 re-estimates each column, but uses the total numbers, rather than proportions.

Table 10: Time Series Results: Using Annual Totals

	Total Thefts	Total Pawn References	Total Pawn-Related Thefts	Total Non-Pawn Thefts
After the licensing fee	800.07 *** (103.04)	117.06 *** (23.34)	159.69 *** (22.10)	640.39 *** (85.98)
R^2	0.578	0.567	0.543	0.558
AIC	665.3	528.7	205.1	648.7

Annual counts from 1800 to 1845 considered ($N = 46$). Omitted category is the period of pawn shop promotion (1800-14). Each specification includes an unreported constant. Standard errors reported in parentheses; *** 1%, ** 5%, * 10% level of significance.

Regarding the panel data set analysis of the Old Bailey, Table 11 presents additional results mentioned/referenced in the text.

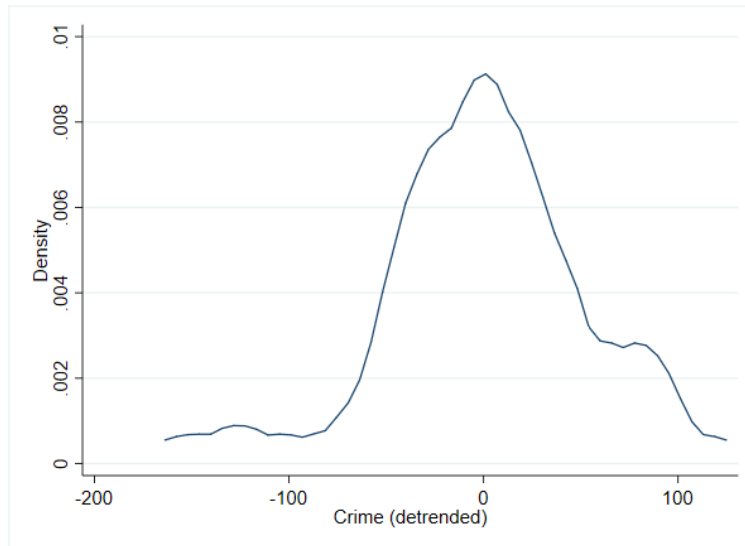
Table 11: Old Bailey: Results Using Annual Panel Data Set

Dep. var.:	Crime (detrended)	Crime (detrended)	Crime (detrended)	Crime (detrended)
Time Period:	1800-14	1800-14	1800-14	1815-45
Data Set:	Treated Crimes	Control Crimes	Full	Full
	[1]	[2]	[3]	[4]
Theft (indicator variable)			1.29×10^{-3} (8518.25)	-29747.85 *** (9162.70)
Year (linear time trend)	10.064 ** (4.546)	2.157 (1.248)	-1.30×10^{-7} (3.33)	7.478 ** (3.54)
Theft x Year			-4.06×10^{-6} (4.74)	16.25 *** (5.01)
constant	-17566.43 * (8214.16)	-3816.22 (2255.68)		
R^2	0.274	0.187	0.000	0.767
N	15	15	30	62

The first two columns present the results of the estimation of the linear time trends prior to the fee's implementation. These are used in the demeaning process. The last two columns Annual counts from 1800 to 1845 considered ($N = 46$). Omitted category is the period of pawn shop promotion (1800-14). Each specification includes an unreported constant. Standard errors reported in parentheses; *** 1%, ** 5%, * 10% level of significance.

Due to violation of parallel trends, the crime data was detrended. As described, separately for both thefts and control crimes, a linear time trend (i.e., a slope and intercept term) is estimated on the pre-1815 data. With these, the fitted values for each type of crime for each year (1800-45) is created. Subtracting it from the actual observations creates our detrended crime variable. Figure 7 graphically depicts the kernel density of this variable.

Figure 7: Distribution of Detrended Crime

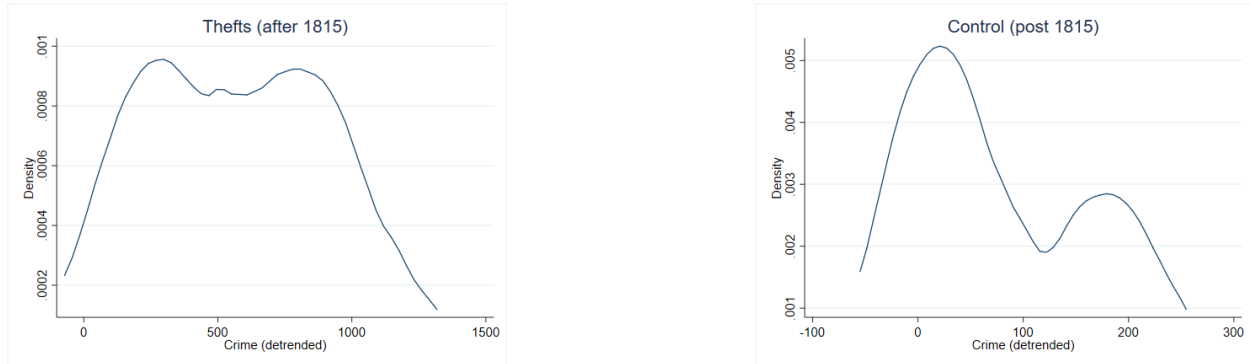


Both the treated and control crime types included for years prior to 1815 only.

Prior to 1815, the detrended crimes each have a zero mean. The change after 1815 reflects the relative

increase in each crime's prevalence. Figure 8 compares graphically the values after 1815 for the thefts and control crimes separately.

Figure 8: Distribution of Detrended Crime After 1815



Both the treated and control crime types included (separately) for years after 1815 only.

8.2 Additional Results Regarding the Covid Analysis

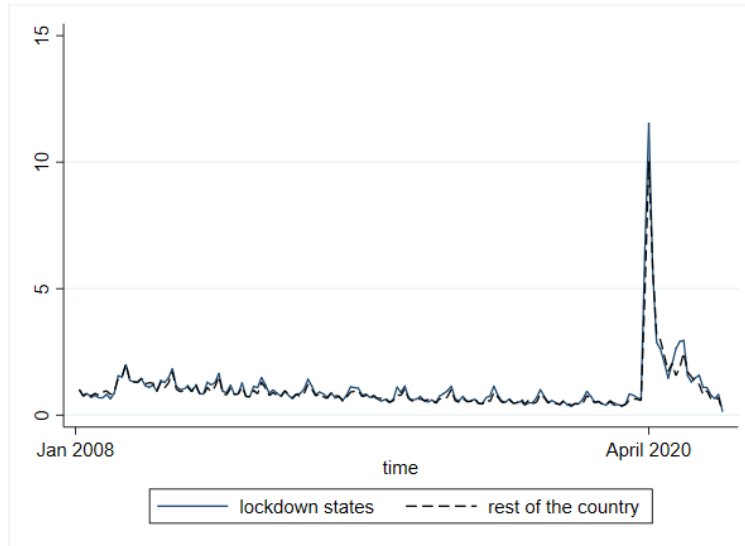
Table 12 separates those states that kept pawn shops open, those that partially shut them down, and those that fully shut them down.

Table 12: State Lockdowns and Pawn Shop Closures

States where pawn shops:		
Remained Open	Partially shutdown	Fully Shutdown
Alabama	California	Alaska
Arizona	Maryland	Colorado
Arkansas	Rhode Island	Delaware
Connecticut		Maine
Florida		Minnesota
Georgia		New Mexico
Hawaii		
Idaho		
Illinois		
Indiana		
Iowa		
Kansas		
Kentucky		
Louisiana		
Massachusetts		
Michigan		
Mississippi		
Montana		
Nebraska		
Nevada		
New Hampshire		
New York		
North Carolina		
North Dakota		
Ohio		
Oregon		
Pennsylvania		
South Carolina		
South Dakota		
Texas		
Virginia		
Washington		

Figure 9 depicts the initial unemployment claims over time.

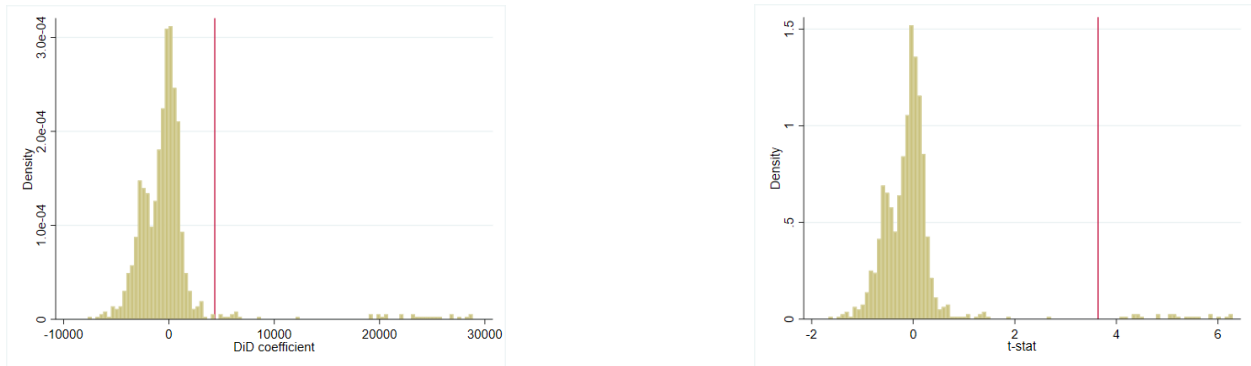
Figure 9: Initial Unemployment Claims Over Time



The time series of initial unemployment claims for each month between January 2008 and December 2020 depicted. The solid line merges the total claims across the six states that shut down pawn shops in spring 2020. The dashed line merges the unemployment claims across the other 44 states. The values are normalized by the January 2008 values. The pairwise correlation coefficient between the two is 0.984.

The text mentions the results of a permutation test. Here, we illustrate. The difference-in-difference term is permuted 1000 times. The coefficient estimate and t -statistics are recorded for each. In Figure 10 the histograms of each are presented. The solid vertical line is the value from the true estimation.

Figure 10: Results from 1000 Permutations



Histograms present the distribution of results from 1000 permutations falsely/randomly assigning a difference-in-difference term $Treated\ State \times Lockdown\ Period$. The left panel is the distribution of the coefficients. The right panel is the distribution of the t -statistics. The vertical lines represent the values from the “true” result.

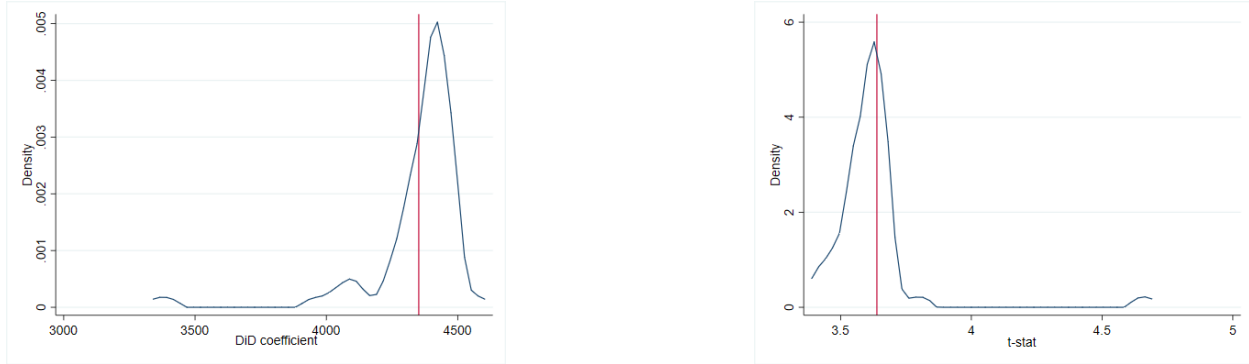
In addition, the main result is re-estimated leaving out one state. This re-estimation is done 50 times – one for each state. Figure 11 depicts the kernel density functions for both the coefficients and the t -statistics.

Table 13: Covid-Related Pawn Shop Shutdowns (Normalized)

	Property [1]	Larceny [2]	Burglary [3]	Murder [4]	Prop. (excluding FL) [5]
Treated Period x Lockdown State	41.95 ** (16.40)	24.41 ** (11.03)	11.23 *** (3.95)	-0.0001 (0.0543)	38.15 *** (16.14)
State Fixed Effects?	Yes	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes	
	Yes				
R^2	0.236	0.226	0.288	0.303	0.298
AIC	96,313.0	91,629.6	72,918.1	10,129.4	91,948.5
DV μ	226.08	160.05	45.34	0.4250	225.49

The variable *Treated Period* is equal to one if and only if the observation occurs in April, May, or June of 2020. The variable *Lockdown State* is equal to one if and only if the observation occurs in one of the six states that shut down the pawn shops. Each specification differs in which crime is used as the dependent variable. Each dependent variable is normalized to measure the number of crimes per 100,000 in population (using annual ACS data). Standard errors are clustered at the state by year level (600 clusters); *** 1%, ** 5%, * 10% level of significance.

Figure 11: Results from 50 Leave-Out Estimations



Kernel densities present the distribution of difference-in-difference coefficients when one state is eliminated from the data set (hence, 50 regression re-estimated). The left panel is the distribution of the coefficients. The right panel is the distribution of the t -statistics. The vertical lines represent the values from the full data set.

The text references results from a normalization of the crime variables. Specifically, the annual population of each state is taken from the American Community Survey. The total number of crimes is divided by this population, and multiplied by 100,000, to obtain a number of crimes per 100,000 people. The main result is re-estimated using these normalized values. Table 13 presents the results.