Asset Forfeiture Patterns as a Function of Negative Shocks to Law Enforcement Budgets: The Case of Louisiana and the Oil Glut of the Mid-2010s[1]

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I. Introduction

During the summer of 2020, at the height of the protests that followed the death of George Floyd, calls for the divestment of resources from police departments rapidly spread across the country. Such proposals, which came to be referred to colloquially as "defunding the police," raise important questions about the ripple effects of budget cuts on a wide range of law enforcement activities. One of those questions is, given a large and unanticipated reduction in criminal justice expenditures, will those affected by such cuts utilize alternative sources of revenue to make-up the resulting shortfalls? There already exists a body of literature to suggest that law enforcement agencies respond to fiscal distress in a number of ways, including by changing policing patterns. Given this, one would expect a large and unanticipated cut to a police department's budget, or to the budget of another law enforcement agency with (1) a claim to some of the revenue generated by the department and (2) the ability to put pressure on that department, to result in that department focusing greater time and resources on the acquisition of revenue through policing activities over which it wields control.

One possible avenue through which police departments might increase their revenues is the process of asset forfeiture. Indeed, in their analysis of issues raised by "defunding the police," Rushin and Michalski (2020) posit that "defunded agencies may also seek additional revenue through potentially harmful means like excessive ticketing and civil asset forfeitures" (Rushin and Michalski 2020, p. 277). Su (2021) raises a similar concern about the effect defunding efforts would have on the revenue-maximization behavior of police departments, though her study focuses on the specific effects of defunding on traffic citations. If we assume that police departments, or those law enforcement agencies sharing in their proceeds and possessing the ability to put pressure on them, may lay claim to some meaningful portion of the

proceeds from the asset forfeiture process, then if they are indeed revenue-maximizers, this theory appears tenable. Under the conditions mentioned, a police department would likely begin exploring the efficacy of the asset forfeiture process as a useful tool in making-up such a budget shortfall.

In the present study, I seek to determine whether or not this hypothesized phenomenon is in fact occurring. To do so, I utilize a negative, real economic shock at the state level as a natural experiment to determine whether or not the ensuing budgetary shortfall drove changes in the use of asset forfeiture. In Section II, I give a brief primer on the process of asset forfeiture. In Section III, I review the relevant academic literature. In Section IV, I lay out both my experimental design and the results it yields. In Section V, I consider limitations to the present analysis. Finally, in Section VI, I conclude my study and offer suggestions for further research focused on this topic and ones similar to it.

II. Primer on Asset Forfeiture[2]

In the United States, asset forfeiture is a process by which law enforcement agencies confiscate and take ownership of the property of a private citizen. The process begins with the seizure stage, in which the property is actually taken from its owner. What happens next depends on the laws in the state in which the law enforcement agency is operating. Under *criminal* asset forfeiture, prosecutors must prove "beyond a reasonable doubt"[3] that the property owner is guilty before any property can be forfeited. Often, prosecutors must also prove that the property in question either was used in the commission of the crime or is a direct proceed of that crime. However, under *civil* asset forfeiture, the property can be forfeited without such a conviction.

[2] Unless otherwise referenced, information in this section comes mostly from the Institute for Justice's website and conversations I have had with Lee McGrath, the Senior Legislative Counsel for the Institute for Justice.
[3] Legal experts typically take "beyond a reasonable doubt" to be mean at least 98% certainty.

That is not to say that in each state with civil asset forfeiture, property can be seized at will without any evidence whatsoever. Though standards of proof in states with civil asset forfeiture are always lower than in states with criminal asset forfeiture, they still vary widely relative to one another.

States with civil asset forfeiture, but that also have conviction prerequisites, tend to have a two-track system for dealing with forfeitures. Under such a system, the legal proceedings for the forfeiture process and the criminal proceedings for the suspect are bifurcated. Also, in many of these states, the conviction standard does not apply to the owner of the property, but rather to individuals who are alleged to have had some connection to the property while committing the crime. In other words, in a civil asset forfeiture system with a conviction prerequisite that does not apply to the owner, that owner may still have his or her property forfeited if it is found to have been connected to another individual in the commission of a crime for which that individual was convicted. For example, if a man borrowed his girlfriend's car to drive to a location at which he initiated a drug deal, in a state of the sort being described, the girlfriend could have her vehicle forfeited if the boyfriend is convicted of some crime related to the drug deal, even though she herself had nothing to do with the deal. That said, regardless of the standard of proof in a given state, once the forfeiture process is completed, the burden on innocent owners to prove that they had no knowledge of or involvement in the crime that led to the forfeiture varies widely from state to state as well. The majority of states place that burden on the owner, while others place it on the government. In a few states, the burden depends on the property in question.

Another margin on which states vary when it comes to asset forfeiture, and perhaps the most germane to this study, is that of the legal regime governing the proceeds from the forfeiture process. According to Knepper et. al. (2020), as of 2020, in every state except the District of

Columbia, Maine, Maryland, Missouri, New Mexico, North Carolina, and Wisconsin, at least some portion of forfeited property and proceeds from the sale of said property winds-up in the hands of law enforcement. In fact, in 32 states, the share for law enforcement agencies is at least 80% (Knepper et. al. 2020, p. 34). Of course, any student of economics will point out that such an arrangement is likely to induce law enforcement agencies to draw on the forfeiture process more than they might otherwise to increase their revenues.

The incorporation of such a profit incentive into the American system of asset forfeiture may or may not have originally been intentional. Prototypes of the forfeiture regimes we now see may have simply been a means of using *in rem* proceedings[4] to address "the practical necessities of enforcing admiralty, piracy and customs laws" (Williams et. al. 2010, p. 10). If the early American law enforcement apparatus had a hard time locating and/or tying to the property the owner(s) of said property flowing through the channels of international commerce, it would make practical sense for the legal system to allow for proceedings to be carried out against the property itself. On the other hand, Williams et. al. (2010, p. 10) have argued that the first forfeiture laws passed by the United States Congress appear to have had their roots in the British Navigation Acts, a set of laws that few would contest were designed to do anything more than enrich the British government and the special interests beholden to it. Regardless of which was the case, by the 1980s, use of the profit incentive to stimulate certain law enforcement actions became more outwardly intentional:

> Modern civil forfeiture use then exploded during the early 1980s as government at all levels stepped up the war on drugs, and Congress and the states created new incentives for the use and arguably the abuse of civil forfeiture. For most of American history, the proceeds from forfeitures went not to the law enforcement agencies responsible for the seizures but to the government's general fund. However, in 1984, Congress amended portions of the Comprehensive Drug Abuse and Prevention Act of 1970 to create the

[4] Such proceedings are brought against property, rather than individuals. This is the asset forfeiture half of the bifurcated process previously described.

> Assets Forfeiture Fund, into which the Attorney General was to deposit all net forfeiture proceeds for use by the Department of Justice and other federal law enforcement agencies. (Williams et. al. 2010, p. 10-11)

In fact, during this period, the use of the profit incentive to mold police behavior was expressly acknowledged by the National Institute of Justice, an arm of the Department of Justice:

> The legislative rationale for allowing law enforcement agencies to benefit from forfeiture seems clear. It is the belief that police departments will be more likely to commit resources to pursue forfeiture of criminal property if the department can gain an automobile for undercover work or cash to supplement the drug "buy fund." Indeed, a few statutes not only allow the police department to keep all forfeited property but explicitly state that forfeited moneys and property cannot be used to reduce appropriations for the police budget. (Department of Justice (National Institute of Justice) 1985, p. 5)

States would soon follow suit by "amending their civil forfeiture laws to give law enforcement agencies a direct share of forfeited proceeds" (Williams et. al. 2010, p. 11).

Today, the American system of asset forfeiture is a true mélange, a tapestry of state-level laws sewn together only by federal involvement, through legislation like the Comprehensive Drug Abuse Prevention and Control Act (CDAPCA) of 1970 and the Civil Asset Forfeiture Reform Act (CAFRA) of 2000, as well as initiatives like the Department of Justice's Equitable Sharing Program. And while the specific mix of guilt thresholds, reporting requirements, proceed recipient designations, and other such nuances can vary widely from state to state, at the core of all these programs is the profit incentive law enforcement agencies face, even in states with pure criminal asset forfeiture and/or where the incentive is much smaller relative to others.

III. Literature Review

There have been a select few direct scholarly assessments of the use of asset forfeiture as a tool for raising revenue. Benson et. al. (1995) were among the first to employ econometric techniques to study the interaction of forfeiture regimes and local budgets. Drawing on data from the state of Florida, they find that confiscations "have a significant positive impact on non-

capital expenditures by police agencies" (Benson et. al. 1995, p. 37), with this effect being stronger in more populous jurisdictions (Benson et. al. 1995, p. 37). A more sweeping, global approach to examining the phenomenon would come later with Baicker and Jacobson (2007), who find that, even though local governments do appear to offset forfeiture revenues via the budgeting process, police still respond to the real net financial incentives that remain. Holcomb et. al. (2018) find that law enforcement agencies in states where the asset forfeiture process is more restricted in scope, and thus offers fewer potential rewards to such agencies, tend to rely more on federal forfeiture sharing programs than agencies in states where the process is less restricted. Worrall and Kovandzic (2008) uncover similar patterns, as do Holcomb et. al. (2011). Worrall (2001) finds evidence that dependence on the forfeiture process is largely predicated upon previous experience with that process, with total proceeds from the forfeiture process being a predictor of future patterns, and that among larger police departments, those with stronger fiscal position are less likely to be dependent on asset forfeiture for revenue. Warchol et. al. (1999) come to more mixed conclusions, noting that their finding that the average value of seized property increasing during the timeframe they examined could be taken as evidence of the federal government actively targeting more valuable property, though they also claim that a focus on conveyance seizures, which they claim have a lower rate of forfeiture than monetary seizures, could indicate that the federal government is more concerned with disrupting drug trafficking than generating revenue.

With specific regards to fiscal distress, Kelly (2019) uses the unemployment rate as a proxy for such distress within a regression framework and finds a positive correlation between it and both the number of equitable sharing requests initiated by local law enforcement agencies and the average value of forfeited property. On the other hand, Billy (2020) finds no evidence

that such distress leads to the use of asset forfeiture as a mechanism for raising revenue. In regards to the role political pressure plays in counterbalancing the revenue maximization incentive, Mughan et. al. (2020) examine the effect of political pressure on forfeiture decision making and find that those more exposed to political pressure (sheriff's offices) tend to seize less, are less responsive to policy changes related to how much forfeiture revenue can be kept by the seizing agency, and utilize the Equitable Sharing Program more than municipal police.

If the presence of asset forfeiture incentives causes law enforcement agencies to focus more resources on areas of law enforcement in which asset forfeiture plays a greater roll, then we should certainly expect patterns of policing and arrests to change as well. Baicker and Jacobson (2007) find that post-offset incentives result in higher drug arrest rates and a higher price of heroin, the latter presumably resulting from interdiction lowering supply. Makowsky et. al. (2019) find evidence that during periods of fiscal distress, the potential rewards offered by asset forfeiture, as well as fines and fees, cause both the racial composition and the offense composition of arrests to change. Shoub et. al. (2012) also find evidence that the incentive structure of fines, fees, and forfeitures leads to unequal policing outcomes along racial margins and that this effect is also magnified during periods of fiscal distress. Kelly and Kole (2016) find evidence that, even though the profit incentive leads to an overall increase in clearance rates across a range of offenses, it also leads to a shifting of policing towards drug offenses.[5] Mast et. al. (2000) and Kantor et. al. (2021) also find evidence of a positive relationship between asset forfeiture and drug arrests, though the latter also find a negative effect on crime. Bishop and Worrall (2009) find no discernible effect on drug arrests. D'Alessio et. al. (2015) dispute the claim that asset forfeiture has increased enforcement disparities along racial lines or led to an

[5] The authors of the paper do not consider these statistically significant results to be economically significant.

uptick in dubious drug enforcement cases, while Barrett et. al. (2017) find little evidence of disparate enforcement of asset forfeiture. Clingermeyer et. al. (2005) use a survey based approach and find no evidence that changes in the values of seized assets alter policing priorities in any statistically significant way, even though they do find evidence that differences in forfeiture laws may affect the types of property seized, as well as use of the federal criminal forfeiture process, and that criminal forfeiture is generally more widely utilized than civil forfeiture, which they believe could be due to the former requiring less revenue sharing than the latter.

There are of course other studies that deal with the concept of "taxation by citation,"[6] but do not focus entirely or at all on the use of asset forfeiture for that end. Makowsky and Stratmann (2009) find that speeding ticket fines are imposed more often in localities in Massachusetts where property tax revenues are lower and in localities where attempts by the town board of selectmen to raise more revenue via property tax increases, which can be taken as a sign of fiscal distress, are rejected by local constituents. Garett and Wagner (2009) look at traffic ticket issuance in North Carolina and find that, while positive changes in local revenue do not appear to have any effect on the number of traffic tickets issued, negative changes do in fact lead to a statistically significant increase in the issuance of such tickets. Su (2020) uncovers a positive relationship between decreases in tax revenue the previous year and increases in per capita fines in the current year in California[7], while Su (2021) finds evidence that sheriff

[6] The first use of this term I can find is a poster reading "No Taxation through Citation" that was used in an anti-red light camera protest in Connecticut, as documented by Bailey (2012). Synthesizing Dieterle (2017) and Foldvary (2015), Carpenter et. al. (2022, p. 4) define taxation by citation as being "when municipalities use their code enforcement powers to raise revenue from fines and fees in excess of what they would collect were they issuing citations solely to protect and advance public safety."

[7] Her finding "that rural counties, general law counties, low-income counties, and Hispanic-majority counties have higher per capita traffic fines than their counterparts" (Su 2020, p. 38) also adds evidence to the notion that financial incentives can change the composition of policing activities.

deputies, who receive local government funding that is partially derived from traffic fines, in the same state raise more revenue from traffic fines following budget cuts than members of the state's highway patrol, who do not receive such funding.

From the perspective of any law enforcement agency, it would stand to reason that one of the drawbacks of utilizing fines and forfeitures to enhance revenue is that it has the potential to generate resentment among the populace being targeted. The greater the proportion of that populace that is composed of local constituents, the more likely it is that the agency in question will face some sort of backlash. Whether it be less cooperation from those constituents in the course of investigations or scolding from local elected officials, who may be hearing it from those same constituents themselves, this channel of generating revenue is not without its costs. For such reasons, it might make sense for agencies to utilize the forfeiture process as a means of raising revenue to disproportionately target individuals who reside outside their jurisdictions. If this were true, the use of forfeiture for raising revenue would essentially function as a means of tax exporting.

One of the first to define and directly analyze the phenomenon of tax exporting, McLure (1967, p.51) defines it as "the loss in real incomes suffered by non-residents of the taxing state as a result of the tax in question." While a more precise definition might place greater emphasis on the conception of tax exporting as a shifting of revenue streams, this one does help in emphasizing the point that for those bearing the burden of the shifted tax, it functions simply as a cost with few accessible benefits or representation. Some have taken this concept and applied it to the analysis of revenue generation within law enforcement. Makowsky and Stratmann (2009) find residency to be an important factor in determining the outcome of an alleged speeding violation, with individuals who are not constituents of the officer's jurisdiction being more likely

to both receive a citation and pay more for that citation. In a similar work, Makowsky and Stratmann (2011) find that when more non-constituents are pulled over for alleged traffic violations, ticket issuances increases. Ralph et. al. (2022) postulate that tax exporting could be one of many benefits to utilizing fines as an alternative revenue stream, though reference is made to Makowsky and Stratmann (2009) as the basis for this claim. All told, the application of tax exporting analysis to revenue generation via fines is slim at best, while explicit application to revenue generation via forfeiture is nonexistent.

A final point to consider in this survey of the literature is that fines and forfeitures are likely to be viewed differently than other, more direct forms of revenue generation by the general public. Hummel (2015, p. 299) raises this point in noting that fees might be preferable to direct taxes on the basis that "they are only incurred when one uses the service" and that fines might be viewed similarly because "they only have to be paid when one breaks the law." Boudreaux and Pritchard (1996, 1996) allude to the notion of civil asset forfeiture as a form of sin tax. If sin taxes are viewed as more acceptable to the general public than other forms of taxation, then forfeiture might be preferred to other forms of taxation by the general public for much the same reason as that offered by Hummel (2015).

IV. Research Design

A. Motivation

Previous studies of the use of asset forfeiture as a tool for raising revenue in periods of fiscal distress have either avoided or failed to take full advantage of the use of natural experiments in assessing this relationship (Graham and Makowsky, 2021). Proxying for such periods of distress certainly occurs with the use of economic and fiscal variables within a regression design and studies of this sort may lend some interesting insights about this

relationship across time and place. However, there has been little in the way of employing a research design intended to assess the impact of a sudden, sustained, and negative shock to government revenue streams on asset forfeiture patterns.

For examining asset forfeiture patterns on the state level, an appropriate shock would need to only affect one state or just a handful of states, so as to keep the pool of potential controls as large as possible. While policy shocks are typically employed in causal inference of this sort, finding a truly exogenous policy shock that affects the fiscal portfolio of only one or a handful of states is a tall order. However, an alternative mechanism for triggering fiscal distress is an unanticipated, negative, real economic shock that causes government revenue streams to diminish and/or sends a signal to lawmakers with influence over the budgetary process to push for budget cuts.

B. Examining the Effect of the Oil Glut of the Mid-2010s on Louisiana's Fiscal Environment

One such shock that only affected a handful of states was the oil glut of the mid-2010s, during which time plummeting oil prices caused economic distress in a number of states that rely heavily on oil production. Consider Figure I, which tracks the price of West Texas Intermediate crude oil prices between January 1, 2010 and December 31, 2019. Oil prices were on a clear upward trend during the 2000s that peaked at the height of the Great Recession, plummeted in the second half of 2008, and began moving back upwards in early 2009, only to plummet again in 2014. Between June 20, 2014 and February 11, 2016, prices would plunge from $107.95 per barrel to $26.19 per barrel, leading to negative, real economic shocks in a number of oil producing states. According to a coincident index[8] published by the Federal Reserve Bank of Philadelphia, states with at least 5 months of negative movement in the index between January

[8] The index is a combination of data on non-farm payroll employment, average hours worked in manufacturing by production workers, the unemployment rate, and deflated wage and salary disbursements.

2014 and December 2016 included Alaska, Louisiana, North Dakota, Oklahoma, West Virginia, and Wyoming (see Table I), all of which have large oil-producing sectors. Of the shocks in these states, Louisiana's began earlier (May 2014), ended later (October 2016), and was the longest in terms of most months of negative movement (17 months) (see Table I).[9] While other states on the list may have experienced shocks of greater magnitude, as measured by the coincident index, none had the persistence of Louisiana's.

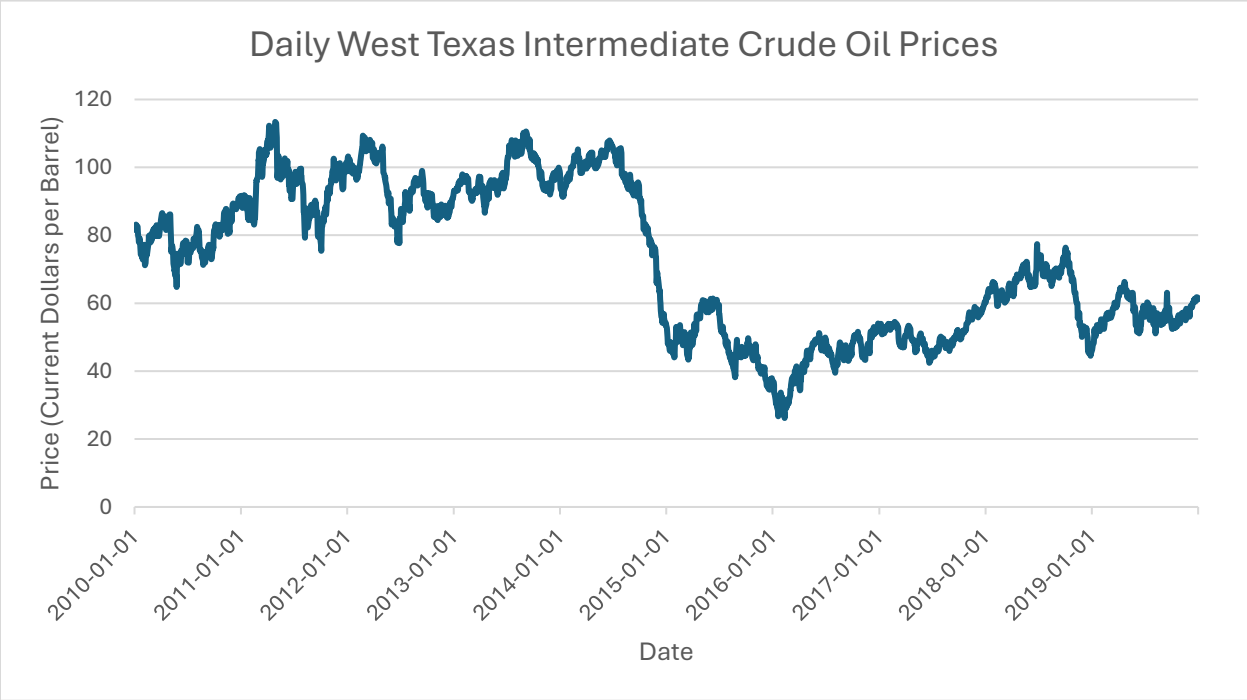


Figure I. West Texas Intermediate crude oil prices from January 1, 2010 through December 31, 2019 in current dollars per barrel

State	Number of Months
Alaska	13 Months
Louisiana	17 Months
North Dakota	10 Months
Oklahoma	9 Months
West Virginia	8 Months
Wyoming	14 Months

[9] The total magnitude of the shock, as measured by the amount of negative movement within the index during the glut, was stronger in other states listed in Table I.

Table I. Number of months of negative movement in the coincident index between January 2014 and December 2016 among states with more than 5 months of such negative movement during the same period

In examining the raw economic data from the period, we are given a portrait of a state suffering the consequences of a heavy dependence, however diminished that dependence may have been from decades past, on oil production. Figure II shows the unemployment rate generally declining coming out of the Great Recession. By February 2014 the rate stood at 5.6%. However, a mere 8 months later, it had climbed to 6.8% and would not fall below 5.6% again until March 2017. Wage and salary data from this period reflects the same decline. Total wages and salaries rose almost exclusively on the downhill side of the Great Recession, as is clear from Figure III. In the first quarter of 2015, this number stood at $97,708,952,000, but fell to $95,977,528,000 by the fourth quarter of 2016. They would not surpass the former high-water mark again until the third quarter of 2017.

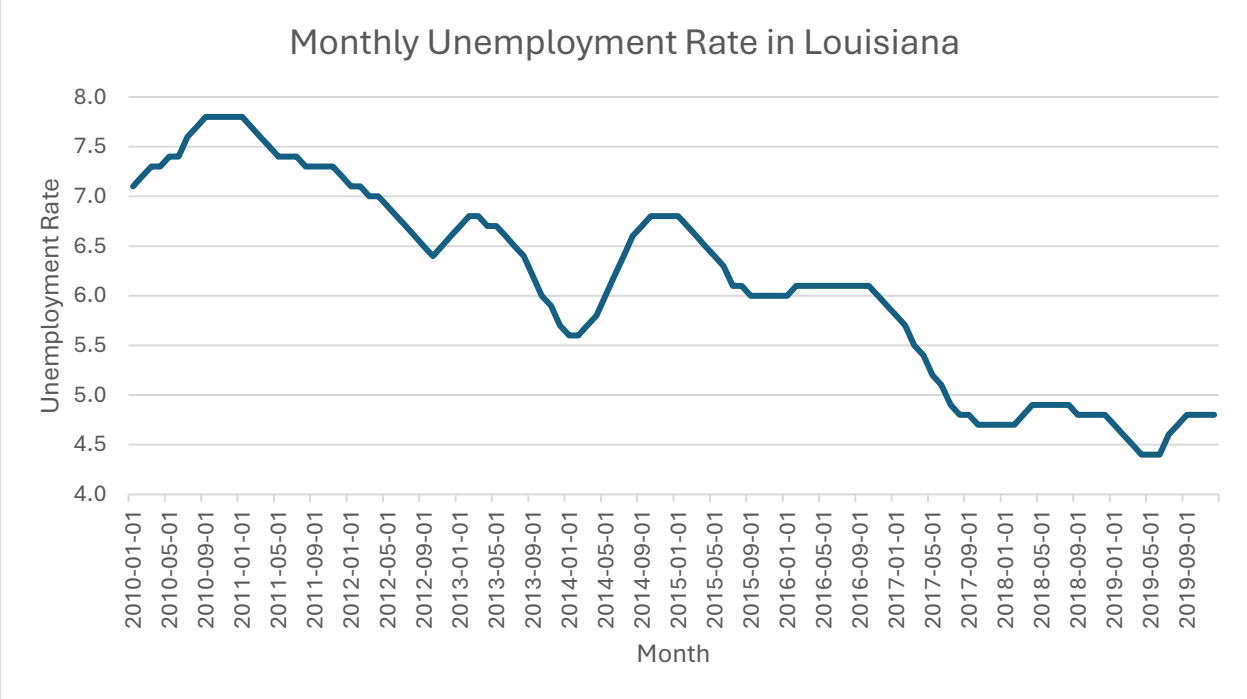


Figure II. Monthly unemployment rate in Louisiana from January 2010 to December 2019.

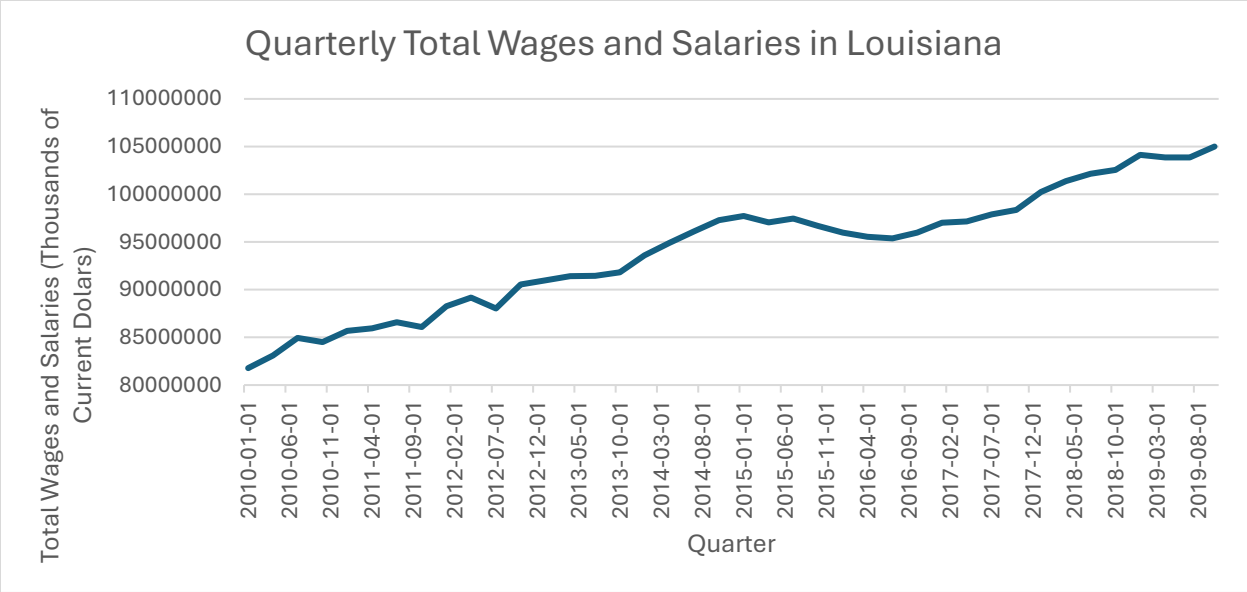


Figure III. Quarterly total wages and salaries in Louisiana from January 2010 to December 2019 in thousands of current dollars

In assessing the effects of this negative, real economic shock on Louisiana's fiscal climate, it is important to understand the powder keg that was the budgetary environment present at the level of the state government during the period in question. In 2002, voters in the state passed a constitutional amendment that was popularly known as the "Stelly Plan." The plan essentially eliminated (over a short period of time) the sales tax on certain items and attempted to make up the lost revenue with increases in personal income tax rates.[10] A few years later, in 2008, Bobby Jindal became Governor and cut taxes six times by early 2009, cuts that included the largest personal income tax cut in the history of the state (The Associated Press 2009). However, as the Great Recession took hold, oil prices began to fall, the post-Hurricane Katrina economic boom waned, and federal relief dollars began to dry-up, state revenues fell and fell

[10] Per the Louisiana Department of Revenue: "Under the Stelly Plan amendment, sales tax on food for home consumption, and sales tax on natural gas, electricity, and water for residential use was lowered from 3.9 cents per dollar to 2 cents per dollar on January 1. Sales tax on these items will be completely eliminated beginning July 1, 2003. To replace state revenue lost by the elimination of these taxes, tax rates in individual income tax brackets were adjusted upward, beginning with tax year 2003. Because returns for tax year 2003 individual income taxes will be filed after January 1, 2004, employer's withholding tax tables were revised and the new rates went into effect on January 1, 2003." (Louisiana Department of Revenue 2003)

harder than they would have prior to the implementation of the Stelly Plan and the Jindal tax cuts. Oil prices would begin to rise again in 2009, but by the eve of the oil glut, the state government's budgetary environment was dry kindling just waiting to be lit. That spark of course came when oil prices began to plummet in 2014.

This reality became all the clearer by the end of the 2014. Per a WDSU-6 article from Christmas Day in 2014:

> With oil prices continuing to plummet, state officials are readying for the possibility they haven't yet hit rock bottom. They may have to deal with another deficit in the current $25 billion state budget before the fiscal year ends June 30, while next year's shortfall could grow even larger. (Sanchez 2014)

And per an article in The New York Times from the following day:

> But Louisiana, which has a smaller and less diversified economy than Texas, is already feeling the sting of the price downturn because it relies on more oil and gas money for its operating budget. Louisiana loses $12 million for every $1 in decline in the annual average price of a barrel of oil, according to Greg Albrecht, the state's chief economist. (Fernandez and Alford 2014)

Reports of the budgetary effects of this oil shock would continue for several years. Consider, from a WWNO article dated May 9, 2016:

> A sudden drop in oil prices last year has brought huge challenges to the state of Louisiana - more than 10,000 layoffs in the oil and gas sector and a $400 million hit to the state budget. Long known for its "working coast" - represented by shipping, fishing and industry in south Louisiana and along the Mississippi River - the downturn brings with it something of an identity crisis. (Wendland 2016)

And from a MarketWatch article dated October 20, 2016:

> In Alaska, poster child of oil-dependent states, lawmakers spent two contentious summer legislative sessions debating how to cover a $4 billion budget deficit. They weren't alone: Plunging oil prices have drilled holes into state budgets - $2 billion in Louisiana, $1.3 billion in Oklahoma, $1.3 billion in North Dakota - that left lawmakers bickering over how to close the gaps. (Ismailidou 2016)

Mention of the effects of falling oil prices in Louisiana, and other oil-producing states, could even be found in 2017:

> These numbers are so unusual because the economy has been growing for almost eight years, and that normally means revenues come in roughly as projected. A multitude of factors explain what's gone wrong this time. First, states that rely heavily on energy taxes like Alaska, Louisiana, Oklahoma and West Virginia have faced budget problems since oil prices began to fall in 2014. (Stone 2017)

However, as one might expect, law enforcement spending in Louisiana does not solely flow from the coffers of the state government. Consider Figure IV, which draws on data from the Bureau of Justice Statistics' Justice Expenditure and Employment Extracts. As Figure IV makes clear, when specifically considering police protection expenditures (minus intergovernmental transfers) in Louisiana between 2004 and 2017, 47% flowed from municipal coffers, while 35% came from those of parishes and 18% from those of the state. This makes sense, as police protection tends to traditionally fall more within the purview of local governments.

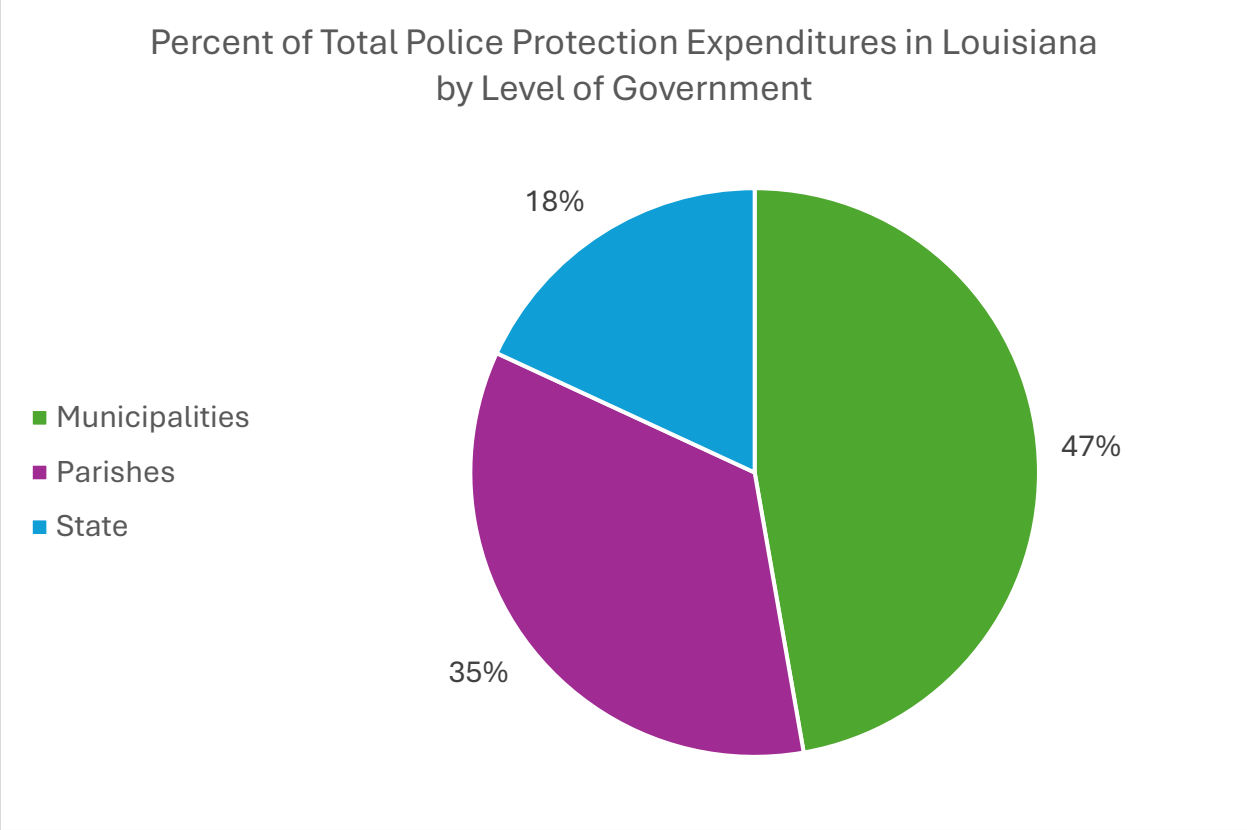


Figure IV. Percent of total police protection expenditures (minus intergovernmental transfers) in Louisiana by level of government between 2004 and 2017

And while one might assume that the incentives created for police departments by most forfeiture laws would thus make municipal governments the most relevant level of focus for the present analysis, that is not necessarily the case. As Blumenson and Nilsen (1998) note, and Williams (2002) reiterates, legal regimes at the state level have been crafted to, in various circumstances, allow for the proceeds of forfeiture to flow to other elements of the criminal justice system, including prosecuting agencies and the courts. Louisiana is no exception. For example, when it comes to certain sex crimes, of forfeited proceeds in excess of those specifically enumerated for expenses such as court costs, 60% are distributed to the seizing agency (or agencies), 20% are distributed to the prosecuting agency, and 20% are distributed to the parish's criminal court where the prosecution happened[11]. As another example, when it comes specifically to seized cash or currency related to public bribery, 55% of such is distributed to the law enforcement agency (or agencies) that investigated the crime, 15% is distributed to the criminal court fund, 25% is distributed to the prosecuting authority, and 5% is distributed to the clerk of court[12]. Given this, a broader measure of criminal justice expenditures is needed as a guide of where to focus the present analysis. As Figure V shows, total justice system expenditures more broadly paint a different picture than total police protection expenditures, with the former being more evenly distributed across all levels of government in Louisiana in the time period in question.

[11] Louisiana Revised Statutes 15.539.1.E
[12] Louisiana Revised Statutes 14.118.E

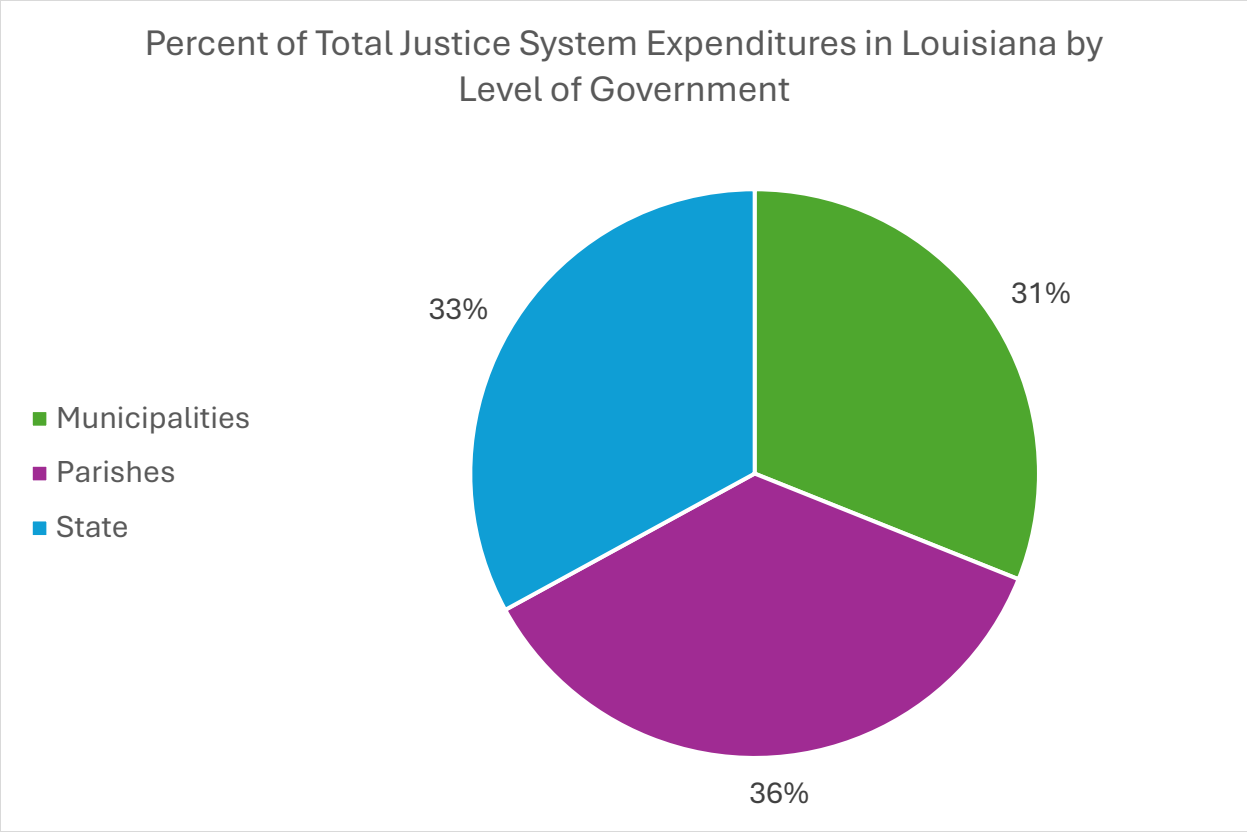


Figure V. Percent of total justice system expenditures (minus intergovernmental transfers) in Louisiana by level of government between 2004 and 2017

Given that the burden of the provision of the state's criminal justice infrastructure is shared nearly equally by all levels of government in Louisiana, it is important to understand the impact of the oil glut on the fiscal environments at all such levels. Drawing on Willamette University's Government Finance Database, which compiles data from the United States Census, I examine revenue patterns at the parish, municipal, and state levels of government during the 2010s. Given that reporting is incomplete at the parish and municipal levels in non-census years[13], a straight comparison to the state level cannot immediately be made. Examining the data on a per-parish or per-municipality level also cannot be done, as non-reporting entities are not

[13] Years not ending in 2 and 7

randomly distributed along the lines of wealth[14]. Thus, the best that can be done is to examine revenue and tax collections on a per capita basis[15]. Doing so reveals, as Figure VI shows, that total revenue (minus intergovernmental transfers), began to fall markedly in 2014, the first year of the oil glut, and only began to recover near the end of the oil glut, while total tax collections stagnated during the glut and only began to rise near the end[16]. It appears that two drivers of the stagnant tax revenues during the glut were the drop in income tax collections at all levels of government, illustrated in Figure VII, and the drop in severance tax revenues at the state level, illustrated in Figure VIII.

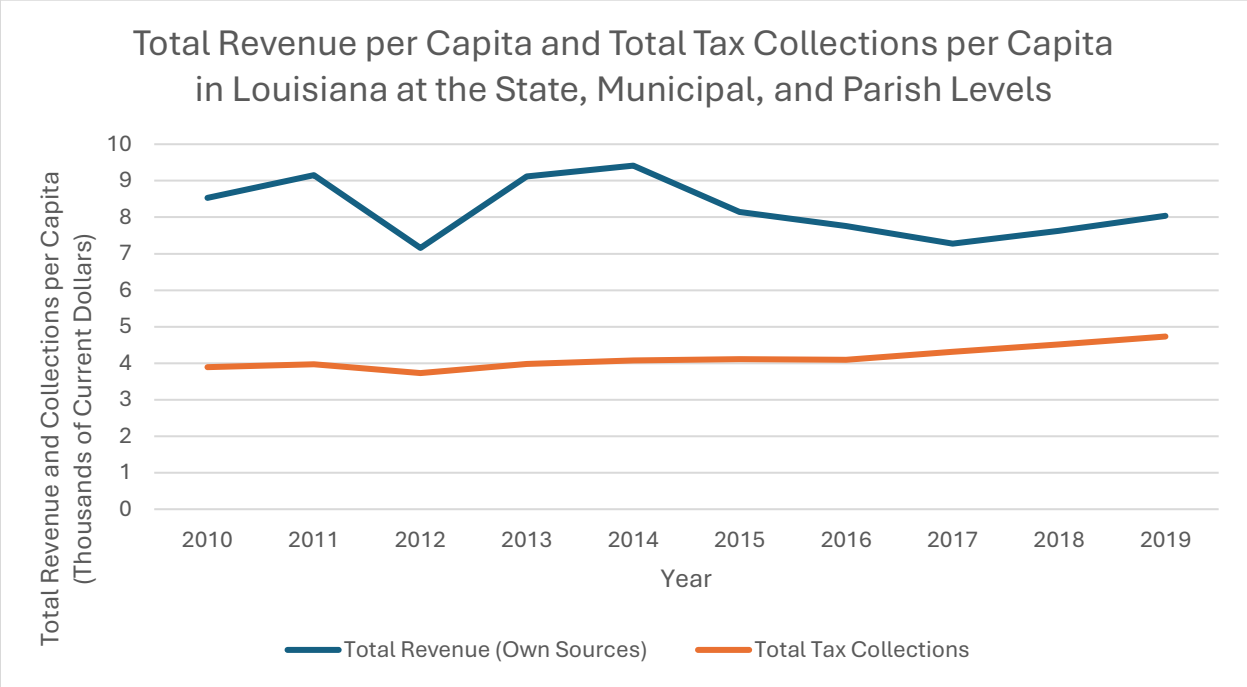


Figure VI. Total revenue (minus intergovernmental transfers) per capita and total tax collections per capita in Louisiana at the state, municipal, and parish levels between 2010 and 2019 in thousands of current dollars

[14] According to Kawika Pearson of Willamette University, non-reporting counties in off-census years tend to skew heavily towards the lower end of the wealth spectrum.

[15] This method was suggested by Kawika Pearson as a more accurate, though still imperfect, means of normalizing the comparisons. Given that the dataset reports the population totals for reporting parishes and municipalities, this makes sense.

[16] Given the reporting issues in off-census years, it is likely that total revenue and total tax collections are understand in 2012 and 2017, even with the current per capita adjustment. Thus, the dropoffs both between 2011 and 2012 and between 2016 and 2017 are likely overstated. Additionally, the increase in tax revenues beginning near the end of the oil glut may have increased partially due to a 1% state sales tax hike that was passed in 2016.

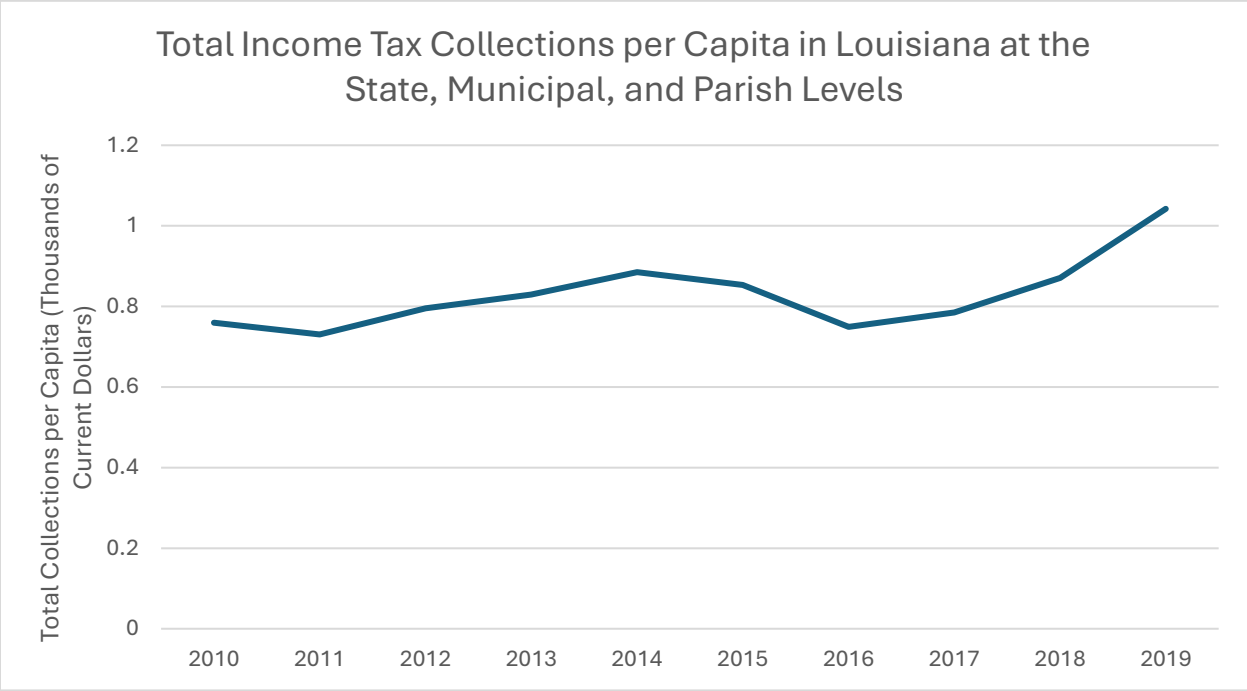


Figure VII. Total income tax collections per capita in Louisiana at the state, municipal, and parish levels between 2010 and 2019 in thousands of current dollars

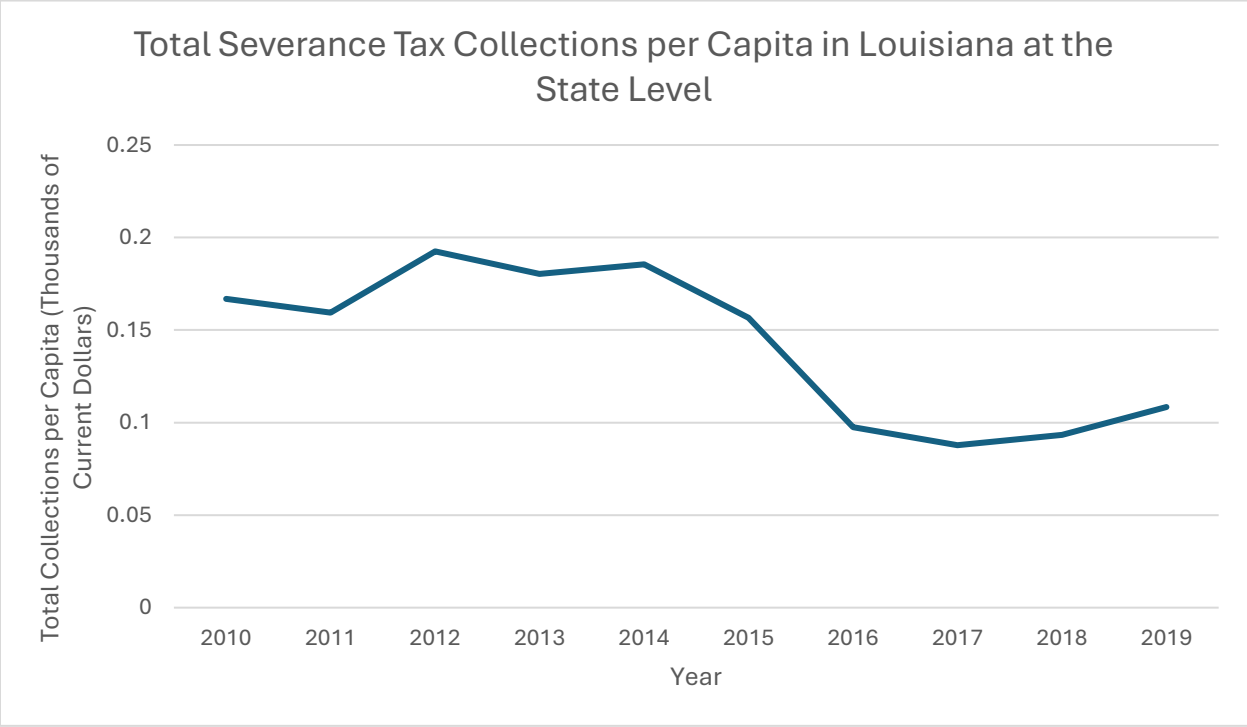


Figure VIII. Total severance taxes per capita in Louisiana at the state level between 2010 and 2019 in thousands of current dollars

Given these plummeting revenues and stagnant tax collections, all levels of government in Louisiana would have been compelled to consider cuts to various programs, including those related to criminal justice. As Figure IX[17] reveals, that is exactly what happened. Total criminal justice expenditures (minus intergovernmental transfers) almost consistently rose between 2004 and 2013, only to fall in 2014 and largely plateau during the glut. Police protection expenditures followed a similar, though not identical path, rising between 2004 and 2013, falling in 2014, and recovering thereafter. Such data would indicate that the economic consequences brought about by the oil glut rippled through the fiscal infrastructures of all levels of Louisiana's governments during the mid-2010s and in fact led to criminal justice budgets being cut during that time. Such would have made various elements of the criminal justice apparatus, and not just the police, desperate to make-up the revenue being lost to such cuts through various methods at their disposal, including asset forfeiture.

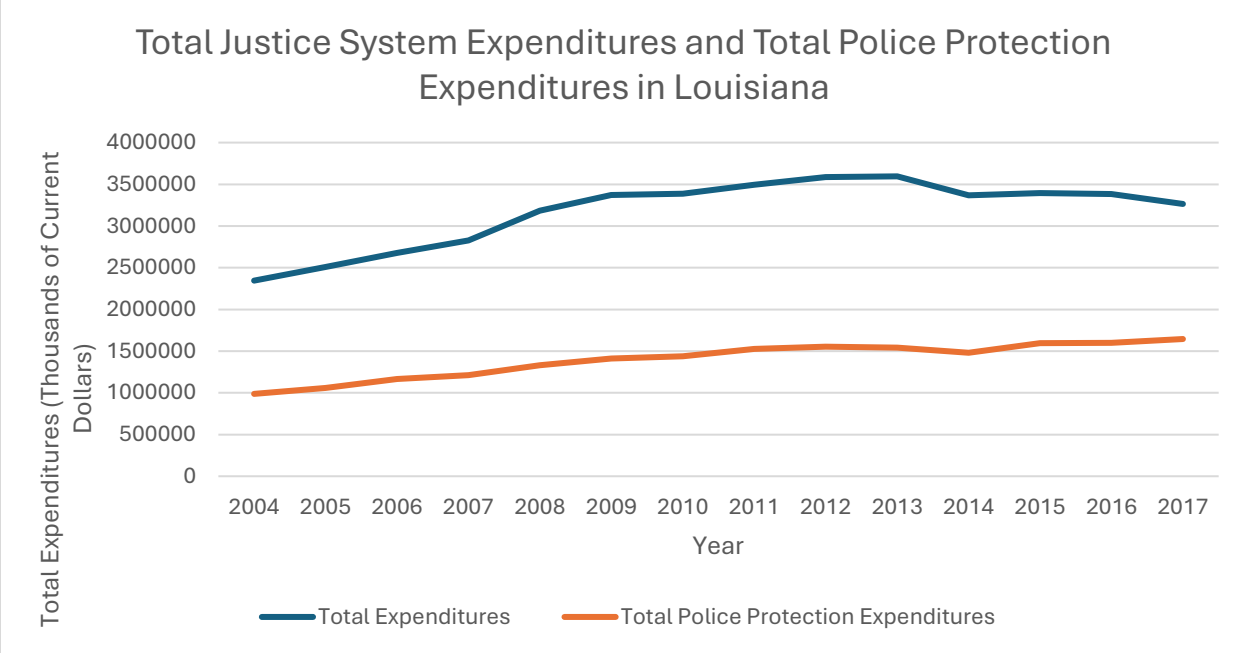


Figure IX. Total criminal justice expenditures (minus intergovernmental transfers) and total police protection expenditures (minus intergovernmental transfers) in Louisiana between 2004 and 2017 in thousands of current dollars

[17] Drawn again from the Bureau of Justice Statistics' Justice Expenditure and Employment Extracts

C. Model

I now turn my attention the empirical set-up for testing to see the effects of this negative, real shock on forfeiture patterns via the budgeting process. I employ the synthetic control method and compare Louisiana to a pool of weighted controls. First employed by Abadie and Gardeazabal (2003) and refined empirically by Abadie et. al. (2010) and Abadie et. al. (2015), the synthetic control method is predicated upon the idea of minimizing the following objective function:

$$\sqrt{(X_1 - X_0 W)' V (X_1 - X_0 W)}$$

For a given observation, X_1 is a vector of covariates for Louisiana, X_0 is a vector of covariates for the control states, W is a vector of importance weights for the control states, and V is a matrix of importance weights that are associated with the control variables. Given that I am utilizing panel data, for a given state in a given year, X_1 would be the measure of forfeitures and covariates for Louisiana, X_0 would be the measure of forfeitures and covariates for the control sttes, and V would be weights based on a series of economic and crime controls I have chosen that help best minimize the mean squared prediction errors. By minimizing the mean squared prediction errors, the goal is to create a synthetic version of Louisiana that closely mirrors the forfeiture behavior of actual Louisiana in the period prior to the 2015 treatment (the pre-treatment period). This allows me to compare synthetic Louisiana to real Louisiana in the post-treatment period and measure any sort of treatment effect of the kind:

$$\alpha_{1t} = Y_{1t} - Y_{1t}^N$$

If there is a palpable disparity between real and synthetic Louisiana in the post-treatment period, we can conclude that there is a significant treatment effect.

In addition to the standard synthetic control approach, I utilize a series of placebo tests. As noted by Galiani and Quistorff (2017), this approach builds on the works of Abadie, Diamond, and Hainmueller (2010) and Cavallo et. al. (2013) to create a statistical package that "i) extends the initial estimation strategy to allow for multiple units that receive treatment (at potentially different times); ii) allows for matching on trends in the outcome variable rather than on the level; and iii) automates the process of splitting pretreatment periods into "training" and "validation" sections" and creates "graphs of diagnostics, inference, and estimate effects" (Galianai and Quistorff 2017, p. 849). Placebo tests with the synthetic control method involve subjecting each of the control states to an equivalently timed shock as that experienced by Louisiana and comparing any treatment effect to the treatment effect in Louisiana in the post-treatment period. Thus, I look to see what would happen if the intervention was applied to all of the states, rather than just the state of Louisiana. If the treatment is significant for Louisiana, then it should not have any meaningful effect in the donor pool states.

D. Data

The Institute for Justice compiles, streamlines, and makes available to the public a series of state-level reports on asset forfeiture revenues and expenses, the former of which is the focus of the present study. To prepare the data for analysis using the synthetic control method, some pruning is required. In doing so, I first exclude those other states with more than 5 months of negative movement in the coincident index between January 2014 and December 2016[18], as states experiencing the same shock cannot be used and controls, and those states with forfeiture totals that, upon visual inspection, appear so erratic that the reliability of the numbers must be called into question[19]. I then utilize a grid framework and examine which combination of

[18] Those removed include Alaska, North Dakota, Oklahoma, West Virginia, and Wyoming.
[19] Those removed include the District of Columbia, Florida, Maryland, New York, and Virginia

controls maximizes the number of observations over the period 2000 to 2018, the years for which data is available in Louisiana[20]. States that do not contribute to said maximization are excluded[21].

It should also be noted that one of the issues with the current dataset is that the unit of observation in the raw data varies between and within states. Some states report revenues from forfeiture activity at the level of the agency carrying-out the forfeiture. Others report at the level of the individual case to which the forfeited property is germane, while still others report at the level of the forfeited property itself. If all states at least reported at the level of the case or the level of the property, some simple division would allow for forfeiture totals per case or average value per forfeiture to be utilized as the outcome variable. However, the absence of uniform reporting means that if I wish to utilize these two variables as outcome variables, I must work with a vastly more limited dataset. Moreover, if the choice of reporting regime is in any way correlated with other variables of interest, then the dataset that can be used for this alternative specification is biased and thus not an effective microcosm of forfeiture activity across the United States as a whole. Thus, I cannot examine forfeiture totals per case or average value per forfeiture.

E. Results

In initially running the synthetic control with Louisiana and the control states, with the total value of forfeitures in a given year as the outcome variable, I discover that Arizona, California, Illinois, and Texas have relatively large mean-squared prediction errors in the pre-

[20] Synthetic controls cannot have any missing observations for the outcome variable in the final dataset. Given that I am working with panel data, this means that there cannot be a missing observation for the outcome variable for any state in any year over the time horizon chosen. This fact is taken into consideration in said grid framework.
[21] Those removed include Alabama, Arkansas, Delaware, Georgia, Idaho, Indiana, Kansas, Kentucky, Maine, Massachusetts, Mississippi, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, Ohio, Oregon, South Carolina, Tennessee, Utah, Vermont, and Wisconsin. This final pruning leaves Arizona, California, Colorado, Connecticut, Hawaii, Iowa, Illinois, Michigan, Minnesota, Missouri, Montana, North Carolina, Pennsylvania, Rhode Island, South Dakota, Texas, and Washington as potential controls.

treatment period. Thus, I exclude them[22]. I derive the weights listed in Table II, as well as the predictor balances listed in Table III. The predictor balances indicate that there is a reasonably strong match between synthetic Louisiana and real Louisiana. The plot of forfeiture totals per year for Louisiana and synthetic Louisiana can be found in Figure X. While certainly not a perfect fit, synthetic Louisiana tracks the totals for real Louisiana quite well in the pre-intervention period (prior to 2015). However, beginning in 2015, there is a clear break in forfeiture totals between synthetic and real Louisiana. While totals in real Louisiana remain relatively stable and even fall somewhat in the post-intervention period, totals in actual Louisiana increase by a significant margin, aside from 2015. Such would indicate that absent the economic shock that began in mid-2014, forfeiture totals should have been significantly lower in the post-intervention period. Additionally, the results of the placebo tests can be found in Table IV, with a plot of the results found in Figure XI. The adjusted p-values in Table IV indicate that the results are statistically significant, at least for the last three years[23]. Meanwhile, Figure XI indicates that only two states rank above Louisiana in terms of the total amount of forfeitures in the post-treatment period, with only one of those making any meaningful leap from the pre-treatment period to the post-treatment period. All told, the value of forfeitures would have been approximately $4,836,693.53 lower per year in Louisiana between 2015 and 2018, absent the shock. This is roughly 1.17 standard deviations higher relative to the average in Louisiana in the post-treatment period.

State	Weight

[22] This additional pruning leaves Colorado, Connecticut, Hawaii, Iowa, Michigan, Minnesota, Missouri, Montana, North Carolina, Pennsylvania, Rhode Island, South Dakota, and Washington as potential controls. This is still the best outcome of the grid framework previously employed, but with Arizona, California, Illinois, and Texas now excluded.

[23] This is of note, as 2015 is the only year in which synthetic Louisiana has higher forfeiture totals than actual Louisiana and is the only year that is not statistically significant in the post-treatment period.

Connecticut	0.003
Hawaii	0.335
Michigan	0.179
Minnesota	0.151
Pennsylvania	0.103
South Dakota	0.229

Table II. Weights of donor states for synthetic Louisiana

Variable	Real Louisiana	Synthetic Louisiana
Forfeiture Total (2002)	4,800,449	4,799,941
Forfeiture Total (2007)	7,439,139	7,437,120
Forfeiture Total (2008)	6,665,129	6,666,662
Forfeiture Total (2009)	8,925,206	8,542,611
Forfeiture Total (2010)	6,387,868	6,385,510
Population	4,497,556	4,509,077
Real GDP (Millions of Chained 2012 Dollars)	233,905.7	215,863.4
Total Employment	2,513,434	2,625,538

Table III. Predictor balance for synthetic Louisiana and real Louisiana

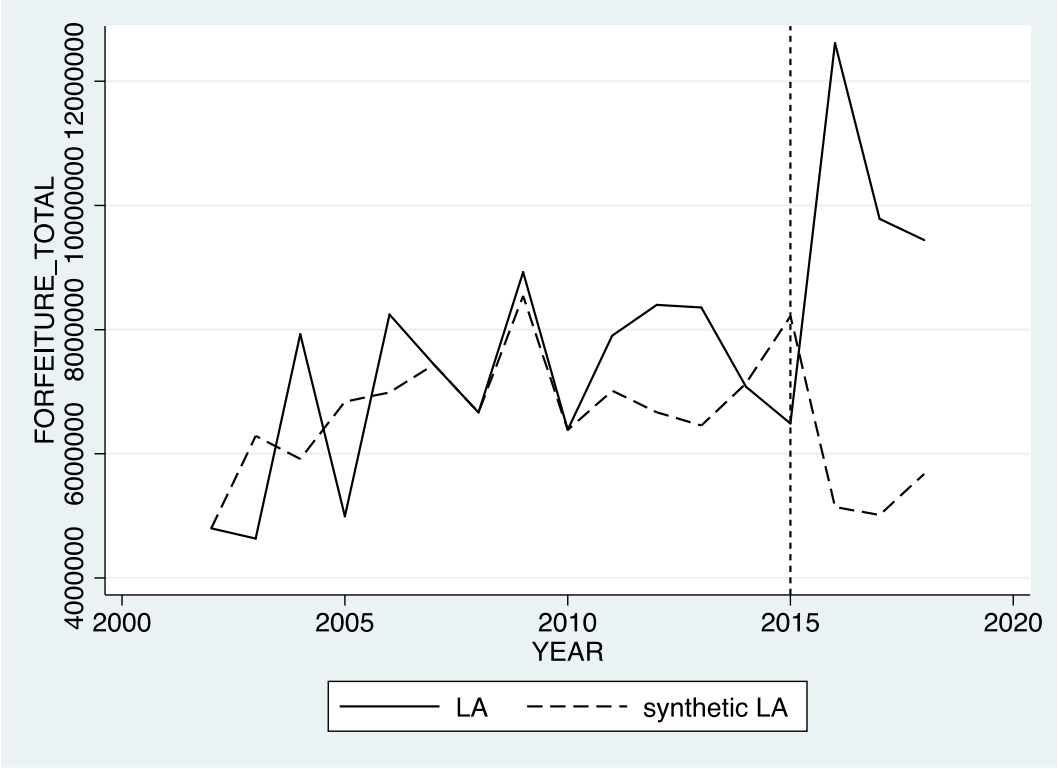


Figure X. Forfeiture totals in Louisiana versus in synthetic Louisiana

Year	Estimate	P-Value	Standardized P-Value

2015	-964,913.9	0.6923077	.7692308
2016	8,807,431	0.0769231	0
2017	6,156,749	0.0769231	0
2018	5,347,508	0.1538462	0

Table IV. Estimates, p-values, and standardized p-values

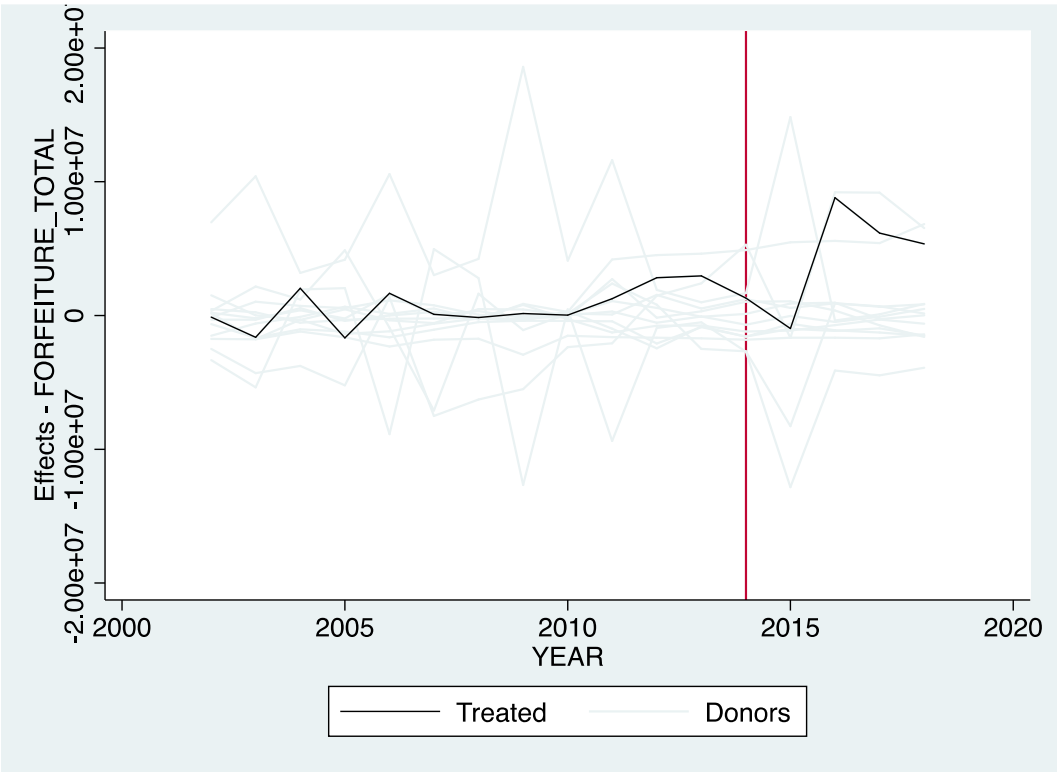


Figure XI. Placebo results for forfeiture totals

As previously mentioned, looking at forfeiture totals per case or average value per forfeiture is of interest to me as well. If police are compelled by budgetary constraints to try to not just generate more forfeitures, but to also economize on time and resources, then we would expect them to not just seek out more opportunities for forfeiture, but also higher value opportunities. Given this, it would make sense for forfeiture totals per case and average value per forfeiture to increase following a shock of this sort. However, given the reporting issues mentioned previously, it is not possible to make an assessment of this sort with the data in question. As such, I instead look at the value of forfeitures per capita in Louisiana. Even though this approach says nothing about forfeiture totals per case or average value per forfeiture, it does

at least provide evidence that population movement is not driving the increase in forfeiture totals found in the initial specification of the synthetic control model being utilized. Here again, after an initial run with Arizona, California, Illinois, and Texas back in the mix, I find that Arizona, California, Illinois, and Montana (rather than Texas) have relatively large mean-squared prediction errors. Thus, I exclude them[24]. I again report the weights (Table V), the predictor balances (Table VI), and the placebo results (Table VII). They indicate a relatively strong fit yet again between synthetic Louisiana and real Louisiana in the pre-treatment period and that the model is again significant in at least the last three years of the post-treatment period. A simple eyeball test of the plot in Figure XII makes it apparent that there is again quite a strong treatment effect at work, with a significant gap between synthetic Louisiana and real Louisiana in the post-treatment period. Finally, as Figure XIII makes apparent, again only two states rank above Louisiana in the post-treatment period. And while both of those states make something of a leap from the pre-treatment period to the post-treatment period, neither is that far above Louisiana in absolute terms. These results provide further evidence that police do in fact seek to utilize the forfeiture process as a means of topping-off revenue in the presence of a budgetary shock. All told, the value of forfeitures per capita would have been approximately $1.06 lower per year in Louisiana between 2015 and 2018, absent the shock. This is roughly 1.18 standard deviations higher relative to the average in Louisiana in the post-treatment period.

State	Weight
Hawaii	0.105
Iowa	0.042
Michigan	0.522
Minnesota	0.300

[24] This additional pruning leaves Colorado, Connecticut, Hawaii, Iowa, Michigan, Minnesota, Missouri, North Carolina, Pennsylvania, Rhode Island, South Dakota, Texas, and Washington as potential controls. This is still the best outcome of the grid framework previously employed, but with Arizona, California, Illinois, and Montana now excluded.

South Dakota	0.031

Table V. Weights of donor states for synthetic Louisiana

Variable	Real Louisiana	Synthetic Louisiana
Forfeiture Total per Capita (2002)	1.07244	1.077875
Forfeiture Total per Capita (2007)	1.732771	1.734944
Forfeiture Total per Capita (2008)	1.511094	1.54564
Forfeiture Total per Capita (2009)	1.986878	1.962115
Forfeiture Total per Capita (2010)	1.405365	1.413322
Population	4,497,556	7,077,818
Real GDP (Millions of Chained 2012 Dollars)	233,905.7	324,777
Total Employment	2,513,434	4,010,603

Table VI. Predictor balance for synthetic Louisiana and real Louisiana

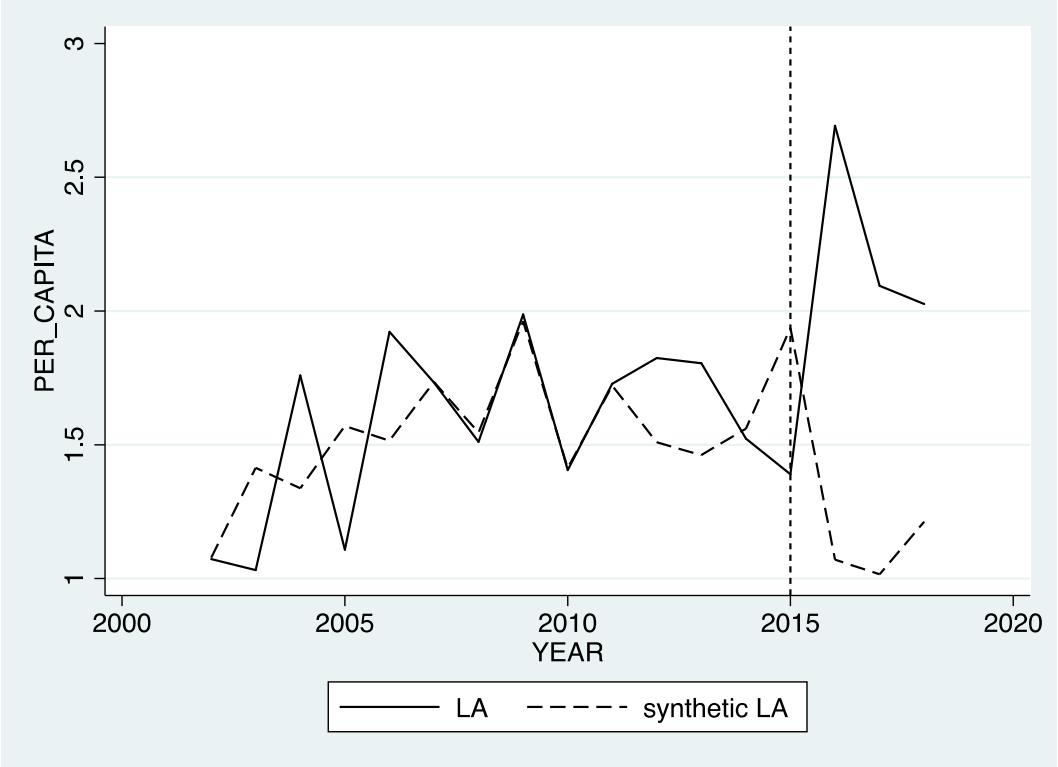


Figure XII. Forfeiture per capita totals in Louisiana versus in synthetic Louisiana

Year	Estimate	P-Value	Standardized P-Value
2015	-0.1957509	0.6923077	0.8461538

2016	1.912556	0	0
2017	1.398352	0	0
2018	1.117473	0.2307692	0

Table VII. Estimates, p-values, and standardized p-values



Figure XIII. Placebo results for forfeiture per capita totals

F. Robustness Checks

For robustness checks, I employ two methods. First, I look to the mean squared prediction errors in both the pre- and post-treatment periods. I take the ratio of the latter to the former and sort the data based on that ratio. The idea here is that the MSPE should be much greater in the post-treatment period relative to the pre-treatment period. Such would indicate that while the synthetic version of Louisiana is a relatively good fit in the pre-treatment period, it is not a good fit in the post-treatment period. This indicates alignment in the pre-treatment period, but divergence in the post-treatment period, which one would expect in the presence of a meaningful treatment. In running these ratios and rank ordering the results, I find that Louisiana

stands at the top of the rank in both the forfeiture totals and forfeiture per capita variations, both before and after the relevant states with large mean-squared prediction errors are excluded. Such would indicate that while the pre-treatment fit is relatively solid for Louisiana, the post-treatment fit is way off, which indicates that the treatment is resulting in some sort of significant effect.

The second robustness check I employ takes into consideration state-level policy reforms related to asset forfeiture. Given that many states implemented asset forfeiture reforms during the 2010s, I consider the effect that said reforms may have had on the projections made. In considering the effect of these reforms, Greenberg (2022) calls into question the efficacy of these reforms, though his analysis lacks statistical inference. Additionally, Blumenson and Nilsen (1998) argue that without reforms to the federal government's equitable sharing provisions, state reforms are essentially meaningless (Blumenson and Nilsen 1998, p. 111). Still, regardless of critiques of the efficacy of state-level reforms related to asset forfeiture, I wish to take into account the possibility that they may have some significant effect on forfeiture patterns. Thus, I employ a series of two sample t-tests to determine whether or not the means of both forfeiture totals and forfeiture totals per capita before and after the year after said reforms went into place[25] are statistically different[26]. This series of tests flags Michigan's reform in 2015 and Minnesota's reform in 2014 as having caused statistically significant differences[27]. Thus, I exclude them as well. As such, I re-run the initial scenarios described, but with both Michigan and Minnesota excluded[28].

[25] This allows for a one year policy effect lag.
[26] I consider only those reforms passed at the state level following the launch of the Institute for Justice's "End Forfeiture" initiative in 2014.
[27] The significance is calculated at the 95% confidence level.
[28] I do not employ the use of another grid framework to determine the optimal balance of years and control states, as the purpose of said robustness check is simply to assess the impact of the previous tests in absence of Michigan and Minnesota as potential controls.

For the initial scenario on forfeiture totals, the weights change (Table VIII), while the predictor balances (Table IX) still indicate a relatively strong, though weakened, match between Louisiana and synthetic Louisiana, as does Figure XIV. However, Table X indicates that this alternative specification results in the difference in forfeiture values per year falling to $3,106,857.55 per year in Louisiana between 2015 and 2018, absent the shock. This is roughly 1.08 standard deviations higher relative to the average in Louisiana in the post-treatment period. The analysis also indicates that the final year of analysis, 2018, is no longer statistically significant. Additionally, as Figure XV indicates, Louisiana has the second-highest post-treatment effect, with the only state above it experiencing an increase that begins prior to the treatment and falls in the post-treatment period.

State	Weight
North Carolina	0.037
Pennsylvania	0.351
Rhode Island	0.180
South Dakota	0.432

Table VIII. Weights of donor states for synthetic Louisiana

Variable	Real Louisiana	Synthetic Louisiana
Forfeiture Total (2002)	4,800,449	4,936,632
Forfeiture Total (2007)	7,439,139	7,121,591
Forfeiture Total (2008)	6,665,129	6,427,319
Forfeiture Total (2009)	8,925,206	7,670,602
Forfeiture Total (2010)	6,387,868	6,546,838
Population	4,497,556	5,284,229
Real GDP (Millions of Chained 2012 Dollars)	233,905.7	257,568.1
Total Employment	2,513,434	3,048,945

Table IX. Predictor balance for synthetic Louisiana and real Louisiana

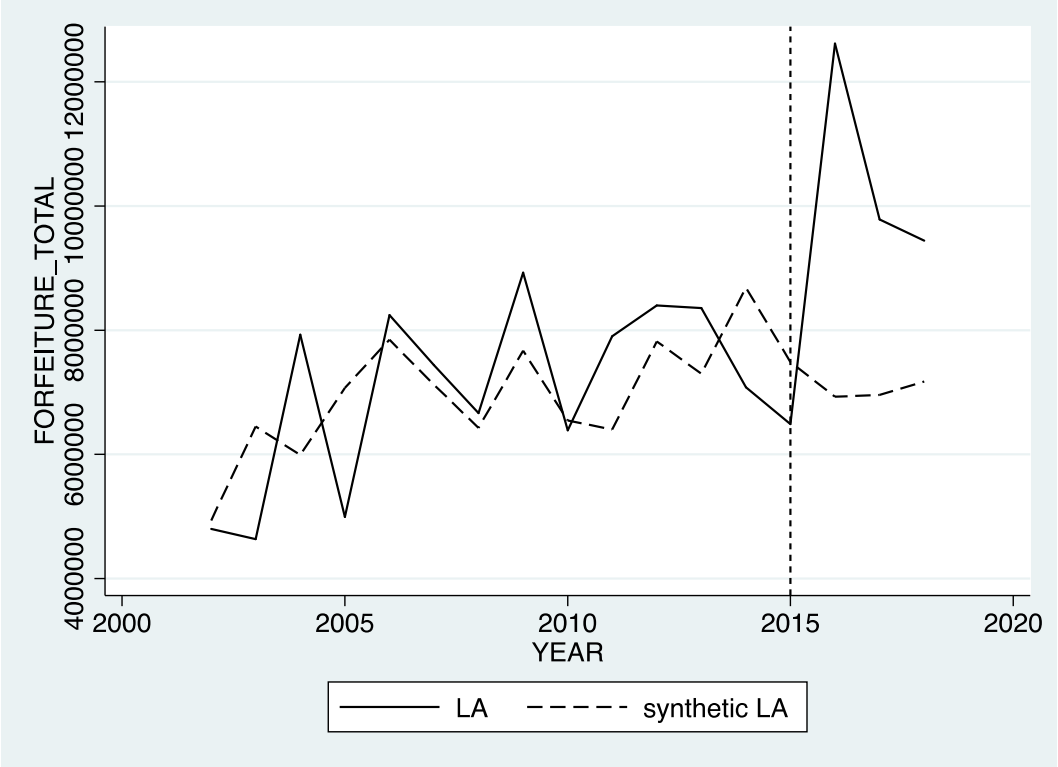


Figure XIV. Forfeiture totals in Louisiana versus in synthetic Louisiana

Year	Estimate	P-Value	Standardized P-Value
2015	-413,025.8	0.7272727	0.9090909
2016	6,608,487	0.1818182	0
2017	3,550,760	0.1818182	0
2018	2,681,209	0.1818182	0.1818182

Table X. Estimates, p-values, and standardized p-values



Figure XV. Placebo results for forfeiture totals

The differences in results for the alternative specification for forfeitures per capita are a bit more palpable, as the predictor balances (Table XII) are even farther off than in the previous robustness specification, indicating that this alternative specification might not create as strong a fit between Louisiana and synthetic Louisiana, particularly along the margins of population, real GDP, and total employment. Still, Figure XVI shows a relatively strong fit in the pre-treatment period and a relatively poor fit in the post-treatment period, indicating that the treatment effect is still strong, while Table XIII indicates that, even though (like in the previous robustness specification) 2018 is no longer significant, there is still a significant effect in 2016 and 2017 in the direction of higher forfeiture per capita for synthetic Louisiana. All told, the value of forfeitures per capita would have been approximately $1.04 lower per year in Louisiana between 2015 and 2018, absent the shock. This is roughly 1.36 standard deviations higher relative to the average in Louisiana in the post-treatment period.

Additionally, I apply the first robustness check to both of the alternative robustness specifications. In both cases, the ratio of the post-treatment mean-squared prediction error to the pre-treatment mean-squared prediction error is highest in Louisiana. Such would indicate that, even in these alternative specifications, in which the fit might not be as strong as in the initial analysis, holistic consideration of the treatment effects in both periods indicates that the post-treatment effect in Louisiana is still strong relative to its pre-treatment fit. Finally, as Figure XIII indicates, there are only two states with larger post-treatment effects than Louisiana, with neither of those states exceeding Louisiana by any large amount in terms of overall magnitude.

State	Weight
North Carolina	0.017
Pennsylvania	0.261
Rhode Island	0.221
Texas	0.501

Table XI. Weights of donor states for synthetic Louisiana

Variable	Real Louisiana	Synthetic Louisiana
Forfeiture Total (2002)	1.07244	0.9420066
Forfeiture Total (2007)	1.732771	1.653998
Forfeiture Total (2008)	1.511094	1.842987
Forfeiture Total (2009)	1.986878	1.884939
Forfeiture Total (2010)	1.405365	1.372433
Population	4,497,556	1.59e+07
Real GDP (Millions of Chained 2012 Dollars)	233,905.7	815,580.8
Total Employment	2,513,434	9,098,178

Table XII. Predictor balance for synthetic Louisiana and real Louisiana

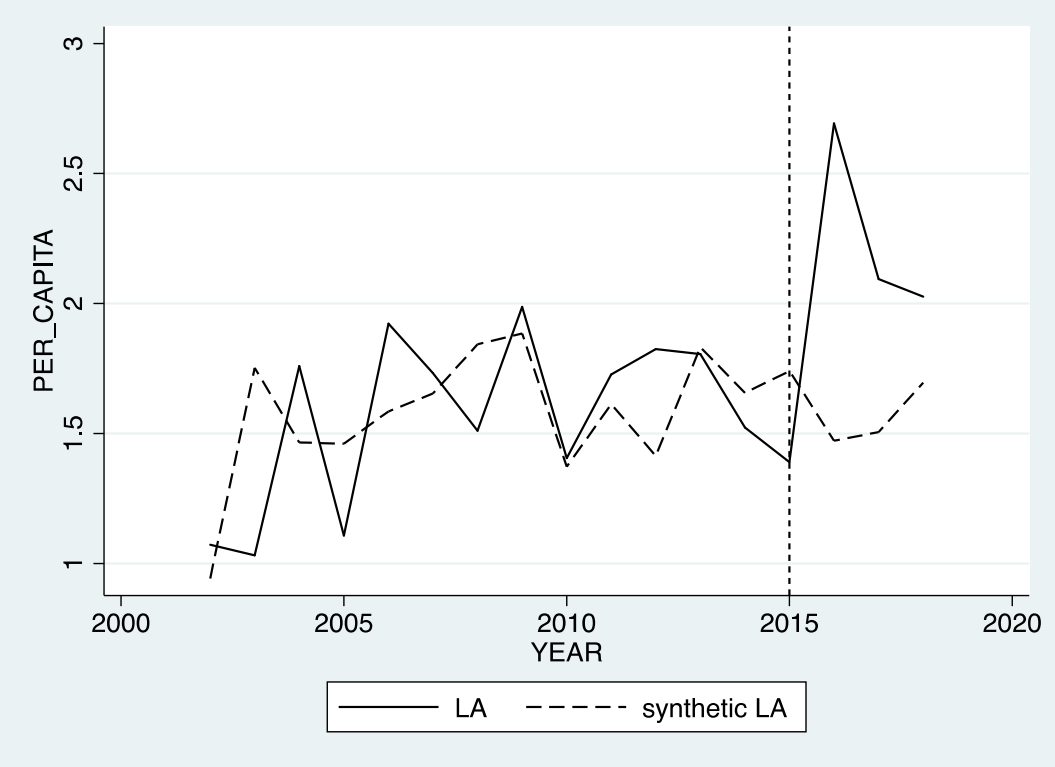


Figure XVI. Forfeiture totals in Louisiana versus in synthetic Louisiana

Year	Estimate	P-Value	Standardized P-Value
2015	0.0734661	0.8181818	1
2016	1.890031	0	0
2017	1.301956	0	0
2018	0.883356	0.2727273	0.1818182

Table XIII. Estimates, p-values, and standardized p-values



Figure XIII. Placebo results for forfeiture totals

As a final robustness check, it is worth considering the effects of the oil glut in the five other negatively impacted states considered. In doing so, I consider graphical representations of the other five states that experienced more than 5 months of negative movement in the coincident index between January 2014 and December 2016. While this would include Alaska, North Dakota, Oklahoma, West Virginia, and Wyoming, data is not available for Alaska and North Dakota, leaving only Oklahoma, West Virginia, and Wyoming. In generating such graphs, I utilize all years of available data for each state. For Oklahoma, as Figure XIV indicates, forfeiture totals primarily rose in the early 2000s, leveled off by the mid-2000s, were streaky in the early 2010s, and followed a generally upward path beginning in 2015, declining only somewhat in 2018. For West Virginia, Figure XV makes it clear that asset forfeiture totals generally rose in a mild and bumpy fashion between 2009 and 2018, though there was a massive increase in 2017. Finally, for Wyoming, Figure XVI indicates that forfeiture totals generally

declined between 2003 and 2014, only to climb thereafter during the oil glut. They would only fall in 2018. Such evidence does not indicate that the oil glut during the period in question unequivocally affected the other states in question. However, the rises in forfeiture totals in each of these three states following the beginning of the oil glut in 2014 could indicate that said glut may have had a positive impact where such totals increased.

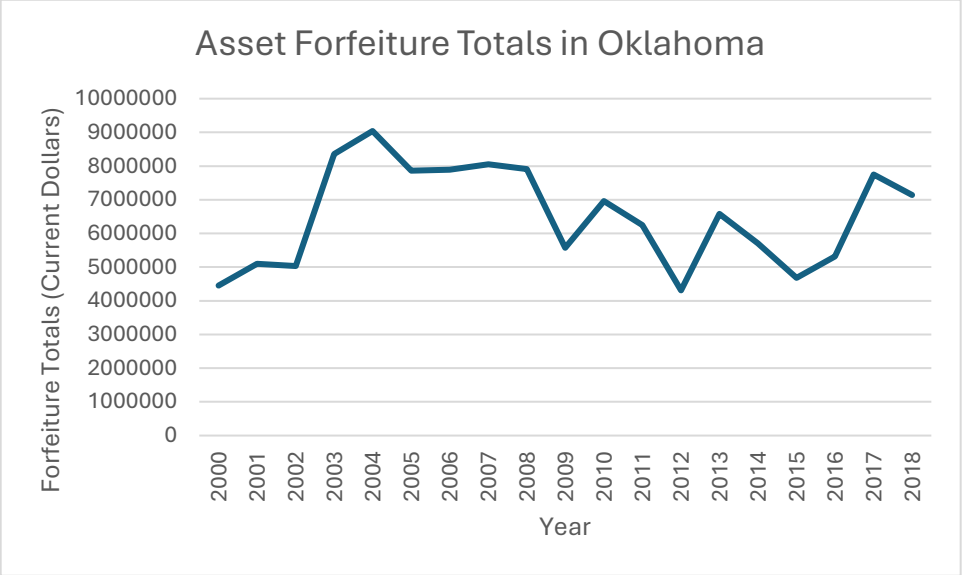


Figure XIV. Asset forfeiture totals in Oklahoma between 2000 and 2018

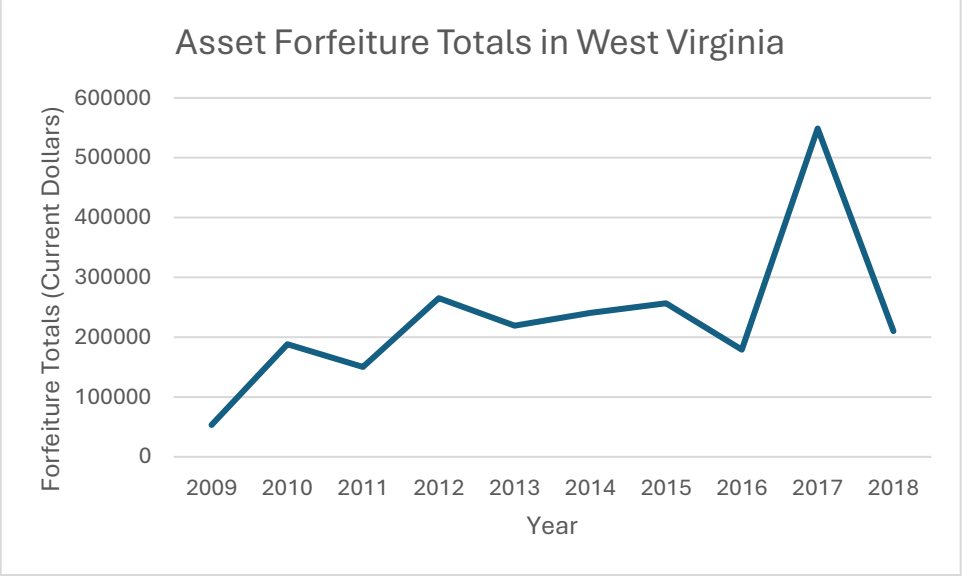


Figure XV. Asset forfeiture totals in West Virginia between 2009 and 2018

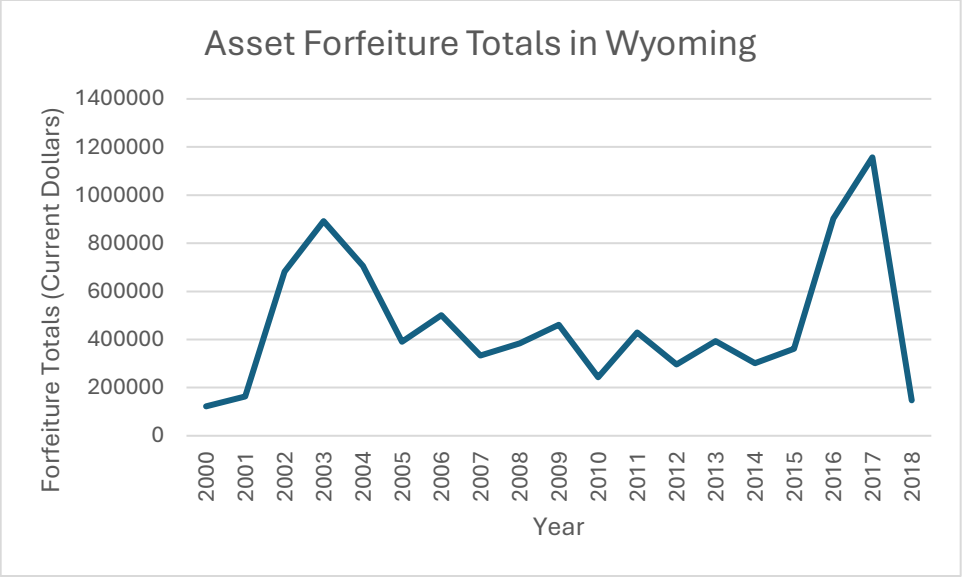


Figure XVI. Asset forfeiture totals in Wyoming between 2000 and 2018

V. Limitations

While the present analysis does indicate that the oil glut of the mid-2010s contributed to a budget crisis in Louisiana that led to budget cuts to justice system expenditures and ultimately increased law enforcement use of the asset forfeiture process to make-up for lost revenues, there are limitations that must be considered. For one thing, as previously mentioned, reporting requirements and practices vary widely across states. This operates along a number of margins. For one thing, given that the dataset is an amalgam of self-reported information from state and local agencies and that reporting requirements for asset forfeiture activity vary both between and within states, the quality of such reports varies as well. This makes interstate comparisons and juxtapositions somewhat tricky, even with the cleaning and streamlining the Institute for Justice employs. Some states report revenues from forfeiture activity at the level of the agency carrying-out the forfeiture, some report at the level of the individual case to which the forfeited property is germane, and others report at the level of the forfeited property itself. Thus, comparisons of forfeiture value per agency, per case, or per unity of property cannot be easily made with the present dataset. What is more, reporting differences between states make even the basic analysis

comparing forfeiture totals used within this analysis shaky. Some states might include forfeiture totals obtained through the Equitable Sharing Program, while other states might not. Still, other states might include forfeitures obtained through the Equitable Sharing Program in certain years, but not in others. Without effective ways to parse the data to control for these reporting inconsistencies, any analysis of the present sort must simply accept that reporting irregularities might not only create inaccurate, yet consistent, totals that might only affect overall levels, but not inter-year comparisons, but also the relative changes across time that would affect an analysis involving comparisons across time, like the analysis in this study. Compounding this issue is the possibility that other reporting practices and case times might very well differ across states, resulting in situations where forfeiture cases involving similar circumstances might end-up being reported in different years for different states, thus making it seem as if similar patterns of forfeiture behavior by law enforcement across states are in fact different when states are compared across time.

Another issue to consider is the pool of potential control states utilized in the present analysis. The pool of potential control states for the initial analysis of forfeiture totals includes Colorado, Connecticut, Hawaii, Iowa, Michigan, Minnesota, Missouri, Montana, North Carolina, Pennsylvania, Rhode Island, South Dakota, and Washington. The same set of potential controls is utilized for the forfeiture totals per capita specification, but with Texas included and Montana excluded. Once robustness checks are applied, both pools narrow to exclude Michigan and Minnesota. While such states seem varied along a number of margins, thus making them an effective microcosm of the rest of the country, we still must consider that there might be margins along which these states are distinct from the rest of the country. And while that might not itself

be an issue if forfeiture patterns are uncorrelated with whatever said margin might be, if there is in fact a correlation, then the pool of potential donor states would be biased.

Finally, it must be noted that while negative movement in the price of oil represents a negative, real economic shock for states with large oil producing sectors, it represents a positive, real economic shock for more diversified states. Indeed, in examining the coincident index utilized in the present analysis, averages of the monthly changes for all all states minus Alaska, Louisiana, North Dakota, Oklahoma, West Virginia, and Wyoming were stronger in the period between 2014 and 2016 than they were in either the period between 2010 and 2013 or in the period between 2017 and 2019[29]. Thus, it cannot be said that the shock in question merely negatively impacted the six aforementioned states, while leaving the others untouched. That said, there are two considerations that must be made when assessing this potential limitation. The first consideration is that the synthetic control specifications used in this analysis indicate that the treatment effect was not merely the result of synthetic Louisiana falling in the post-treatment period, which could have been partially influenced by falling oil prices resulting in less forfeiture activity in the control states, but also by a massive increase in forfeiture activity in real Louisiana in the post-treatment period. Thus, while forfeiture activity in synthetic Louisiana might have theoretically been higher in the absence of falling oil prices, it is highly likely that this would have only diminished the effect to some degree, rather than negating it entirely. The second consideration is that while movement may have been stronger in the period of 2014 to 2016 for the potential control states than in the other two periods mentioned, it is likely that a major portion of the catalyst behind this positive movement was unrelated to the oil glut. The period of 2014 to 2016 was a period of recovery from the Great Recession, meaning that movement in the

[29] The District of Columbia is not included in the coincident index.

coincident index for those years may simply been the economy as a whole springing back towards its longer-term trajectory, which would been largely unrelated to the oil glut, even if it was partially responsible.

VI. Conclusions

My aim in the present study has been to judge the effects of a large and unanticipated drop in a given jurisdiction's law enforcement budget on forfeiture patterns within that jurisdiction. One theory is that law enforcement agencies experiencing fiscal distress will make greater use of fines, fees, and forfeitures as a means of filling their budget holes. There have been some attempts to measure this phenomenon, but in doing so, none have taken full advantage of a natural experiment, much less a synthetic control framework. Thus, I sought out a natural experiment and postulated that a negative, real economic shock of strong enough magnitude would be expected to result in constricted revenues and, ultimately, reduced law enforcement budgets. If such a shock was localized, comparison via synthetic controls could be useful. In seeking out such an experiment, I discovered more about the oil glut of the 2010s and how it had a profound negative impact in states with heavy oil production, while having either a negligible or small positive effect in most other states. Thus, I chose Louisiana and found that forfeiture patterns did in fact change following the oil-fueled turmoil of the mid-2010s. While the synthetic models I derived were certainly not perfect fits, they were quite strong. Ultimately, they revealed what I expected them to reveal: that following the fiscal distress fueled by the real economic shock, the total amount of forfeitures and the total amount of forfeitures per capita both increased relative to what they should have in the absence of this shock. Such models held up relatively well in the presence of alternative robustness specifications and would seem to be strong evidence in favor of the notion that police do in fact utilize the forfeiture process as a means of

revenue generation in times of fiscal distress. Such holds unique policy implications for those considering defunding their police departments to some degree or another. I would recommend that future research build on the use of natural experiments to measure the phenomenon of "taxation by citation" and look for means of deriving data that could be utilized to assess forfeiture totals per case or average value per forfeiture.

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