Policing for profit? Evidence from post-Ferguson reforms in Missouri

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Abstract

In the United States, fines and fees from policing can be a major source of municipal revenue. Critics argue that this incentivizes law enforcement to maximize collections rather than safety. We investigate this issue in the context of reforms in Missouri designed to curb policing for profit after the 2014 shooting of Michael Brown. We document large aggregate declines in per-capita collections (57 percent) and traffic stops (26 percent) immediately after the shooting. The aggregate decline is concentrated in municipalities raising significant revenue from collections before 2014 and in locations with the motive and opportunity to engage in policing for profit. Motivated by these findings, we focus on a reform that limited revenue incentives by capping the share of a municipality's general revenue from fines and fees at 20 percent. We find that municipalities treated by the reform significantly reduced both collections and stops, and the reform can explain as much as 50% of the post-Ferguson declines in collections. These limits on policing for profit also had racial implications: As a result of policing disparities at baseline, the overall reduction in the likelihood of a traffic stop for black drivers in Missouri was seven times larger than the corresponding reduction for white drivers. Taken together, our findings highlight how revenue motives in law-enforcement can distort the criminal justice system and contribute to inequality across racial groups in the frequency and nature of their interactions with the police.

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1 Introduction

Although the criminal justice system is an important public service, it also represents a significant source of potential government revenue through the collection of fines and fees. Critics have argued that an over-reliance by municipal authorities on law enforcement as a source of revenue creates incentives at odds with the core objectives of policing, which typically involve public safety (Tax Policy Center, 2025).[1]

This practice, termed "policing for profit," gained considerable attention after the fatal shooting of Michael Brown in Ferguson, Missouri, in August 2014. Indeed, the ensuing Department of Justice (DOJ) investigation focused heavily on revenue generation, noting both budgeted increases in revenues from fines and overt pressure from city officials to fulfill those projections by writing more tickets. Indeed, a few months prior to the shooting, the mayor of Edmundson, a town close to Ferguson, wrote to the Police Department and *explicitly* linked upcoming police salary raises to revenue from tickets.[2]

More broadly, the decentralized nature of policing in the US creates conditions that are ripe for exploitation. The mayor of Edmundson governs fewer than 1,000 residents in a small geographic area measuring less than one square kilometer; yet, roughly 100,000 vehicles pass through the municipality daily via a small strip of highway I-70. This provides both an opportunity and a motive to finance local government by aggressively targeting these nonresident drivers, a practice known as "tax exporting".

The conditions that give rise to these incentives are not unique to Edmundson. There are 18,000 different state and local law enforcement agencies currently operating in the US and, according to Department of Justice data, "The most common type of agency is the small town police department that employs 10 or fewer officers."[3] These small town police departments are likely to have many interactions with non-residents, who cannot hold local officials accountable for these practices by voting in local elections. Indeed, many municipalities in the United States raise a substantial share of their operating budget from law enforcement, with over 700 municipalities collecting at least 10 percent of their revenue from fines and fees in 2019.[4]

In this paper, we examine whether and how statewide reforms enacted after the shooting of

[1] For example, the mission statement of the New York City Police Department is, "to enhance the quality of life in New York City by working in partnership with the community to enforce the law, preserve peace, protect the people, reduce fear, and maintain order." (CITE: https://www.nyc.gov/site/nypd/about/about-nypd/mission.page)

[2] In an April 2014 memo sent to the Edumundson police department, the mayor wrote: "I wish to take this opportunity to remind you that the tickets that you write do add to the revenue on which the P.D. budget is established and will directly affect pay adjustments at budget time."

[3] National Sources of Law Enforcement Employment Data, US Department of Justice, 2016

[4] https://www.nytimes.com/2021/10/31/us/police-ticket-quotas-money-funding.html

Michael Brown affected policing in Missouri.[5] We seek to address two key research questions. First, how did revenue-driven policing change after Ferguson? Were these changes linked to places that were engaged in, or had incentives to engage in, policing for profit? Second, can any changes be explained by subsequent reforms to the policy environment? Or, are they more consistent with a "post-Ferguson" shift in norms around policing that occurred in light of the increased scrutiny?

To answer these questions, we assemble a novel data set that unites a detailed accounting of municipal court collections with rich measures of police behavior. These data span the years before and after the shooting in Ferguson, as well as the subsequent policy reforms. Our two key outcomes, both measured on an annual basis, are per-capita municipal court revenue collections and police traffic stops per resident. We supplement these data with information on municipal government finances, GIS data covering the network of interstate highways, traffic information from the federal Department of Transportation, and demographic characteristics as measured in the 2010 U.S. Census.

Across all municipalities, we document a sharp decline in both collections per-capita (57 percent) and stops per resident (26 percent) in the aftermath of the shooting. These declines are concentrated in places that were raising large amounts of per-capita revenue via fines and fees prior to Ferguson, suggesting that municipalities likely to be engaged in policing for profit were most affected. More directly, we find that the aggregate declines are also concentrated in municipalities with the incentive (via tax exporting) to engage in revenue generation: municipalities with an interstate highway presence experience drops that are over 50 percent larger than average, and we find similar effects in places with a significant volume of highway traffic. In all cases, these large and sudden declines occur despite the fact that per-capita revenue and stops exhibit nearly identical pre-trends across municipalities that were more or less exposed prior to the shooting. Thus, it is not the case that places that engaged in (or were likely to be engaged in) policing for profit declined simply due to a pre-existing trend.

Next, we explore whether specific policies intended to curb policing for profit in Missouri contributed to the drop. Our focus is on a bill known as SB5, which placed a 20 percent cap on revenue retained by the municipality. More specifically, the law required that any revenue from

[5]While our empirical analysis focuses on a single state, we believe that the lessons from this analysis extend beyond the borders of Missouri, which is not unique in its approach to local government revenue sharing. Indeed, the majority of states allow municipalities unrestricted discretion in the allocation of fines and fees, including allowing municipalities to explicitly budget for future revenue obtained from them. In fact, there are currently only two states that explicitly limit the share of local government revenue that can be generated from police activity. For a summary of current state-level laws, see: https://ij.org/report/municipal-fines-and-fees/municipal-fines-and-fees-data/?id=B. Thus, the policy variation explored in this paper is of direct relevance for understanding the impact of similar laws under consideration in other states, and for efforts aimed at national change.

fines and fees in excess of 20 percent be remitted to the state, limiting the direct incentive for local municipalities to engage in policing for profit. Effectively, the law created a sharp reduction in the local revenue share of a marginal ticket, moving it from one to zero. Under a revenue motive, this change should lead to dramatic reductions in policing. Under a safety motive, by contrast, there should be no changes in policing. Consistent with a revenue motive, we find significant relative declines in both collections ($144 less per resident) and stops (39 fewer stops per hundred residents) in treated municipalities (i.e. those with a share of revenue from policing in excess of 20 percent at baseline).

The magnitude of the effect is large. The point estimates suggest that the affected municipalities reduced per-capita collections by $144 (SE = 27.1) relative to a baseline collections of $274 per resident in the average treated municipality.[6] Taken at face value, our estimates imply that SB5 was responsible for as much as 50% of the overall decline in collections over the time series, an amount totaling over $41 million during the subsequent five-year period.

While the large SB5 treatment effect is consistent with the change in incentives leading to a corresponding change in police behavior, it is also potentially consistent with the increased public scrutiny triggered by the shooting of Michael Brown differentially affecting municipalities that were over the 20 percent threshold on retained revenues. For policy, it is important to distinguish between these two potential mechanisms.

We argue that the cap on retained revenue is the primary mechanism and offer four pieces of evidence in support of this claim. First, the results are robust to comparisons within groups that arguably had more or less scrutiny but were differentially treated by SB5. More concretely, our findings are similar both inside and outside of St. Louis County. We expect the counties outside of St. Louis, located far from the center of controversy and where over 93 percent of residents are white, to be substantially less exposed to the controversy and any associated increases in scrutiny of police. Likewise, our effects are similar when restricting to areas with interstate highways, which, as noted above, had the incentive to engage in policing for profit at baseline, and our results are also similar when restricting to areas that were raising the most revenue from policing at baseline. In addition, our results are robust to time-varying controls for being in the St. Louis metro area, where Ferguson was located, and the share of black residents in the municipality, a variable that we expect to be a strong predictor of which police departments were most exposed to public attention after Ferguson given the racial salience of the controversy. Our results are also robust to controls for exposure to two other policy reforms, which we interpret as further proxies for scrutiny following Ferguson.

Second, we note that the dynamics of the treatment effects are consistent with SB5. While

[6]The average municipality in our data collected $86.30 per resident at baseline.

there is a small drop in collections in the year after Ferguson, the largest drop occurs during 2016, the year in which SB5 was implemented. Third, in terms of magnitudes, the documented 67 percent decline in revenue greatly exceeds the associated 18 percent decline in stops. If our results are driven by police reducing effort and simply making fewer stops after Ferguson, we would expect similar proportional declines. Our results suggest that revenue-per-stop was the key margin of adjustment, which is consistent with the reduced incentive for revenue driven policing. Fourth, when decomposing stops into stops resulting in citations, which generate revenue, and stops resulting in warnings, which do not generate revenue, our decline in stops per capita following SB5 is driven exclusively by a reduction in citations, with no evidence for a reduction in warnings. This evidence is consistent with the weakening of a revenue motive in treated municipalities, relative to control municipalities. Thus, taken together, we argue that the data are more consistent with the limit on retained revenue and the corresponding sharp changes in incentives to engage in revenue generation as the primary mechanism behind the decline in revenue.

The DOJ investigative report also argued that revenue motives were likely an important source of racial disparity in a system already rife with them, citing both statistical evidence of disproportionate harm and anecdotal, but direct, evidence of racist statements by local police and city officials (DOJ, 2015). Given this focus of the DOJ report on the racial implications of policing for profit, it is natural to ask whether this massive decline in collections had different implications for black and white residents. Therefore we conclude our analysis with an exercise that explores how the cap on retained revenue impacted racial equity. We find that the reduction in stops was proportional for black and white drivers (i.e. there was a 27 percent reduction in stops of both black and white drivers). Despite this, we document that the cap on retained revenues differentially benefited black Americans, relative to white Americans, given both their higher rates of being stopped at baseline and the fact that Blacks are more likely to live in these municipalities that were engaging in policing for profit at baseline.

2 Related Literature

Our work fits into a literature on policing for profit. The existing literature studies revenue-driven policing via incentives created by general fiscal conditions and preexisting laws. For example, Makowsky and Stratmann (2009) show that police in Massachusetts are more likely to issue tickets to out-of-town drivers and when fiscal conditions are tight. Garrett and Wagner (2009) also document an important role for fiscal conditions in North Carolina. Makowsky et al. (2019) using national data, document that certain types of arrests are higher when states allow for retention of forfeitures and under local deficits. Our contribution involves the study of new

laws and other policy reforms specifically designed to limit policing for profit. Our identification approach, based upon a differences-in-differences strategy, is also distinct from these studies, which exploit cross-state variation in forfeiture laws and panel variation in local fiscal conditions.

Our research is also related to research on pattern or practice investigations, which may result in changing the "price for individual officers of making type 1 errors," as described in Devi and Fryer Jr (2020). The reforms that we study, by contrast, only apply to certain policing activities (e.g. fines for traffic violations) and are primarily aimed at revenue generation. In this sense, the two projects are complementary, with Devi and Fryer Jr (2020) focused on federal investigations and our project focused on state-level reforms around revenue generation. That said, the possibility that a reduction in policing in response to the events in Ferguson (and subsequent investigations) will drive our results, rather than the policies we aim to study, is an important consideration. We return to this point in more detail below.

3 Background

On August 9, 2014, Michael Brown, an unarmed Black 18 year-old man, was killed by police officer Darren Wilson in Ferguson, Missouri during a confrontation after Brown stole cigarettes from a convenience store. Protests against Brown's shooting began on the following day, with some protesters confronting police and damaging storefronts near the location of Brown's death. Additional protests continued throughout the month of August, restarted in November 2014 after a grand jury declined to indict officer Wilson, and again in August 2015 on the anniversary of Brown's death. Brown's death and the subsequent protests attracted international attention and press coverage.[7]

The U.S. Department of Justice investigated both the details of the shooting and the practices of the Ferguson police department, issuing two reports during March 2015. The Ferguson police department report highlighted a revenue motive and policing for profit, concluding that "Ferguson's law enforcement practices are shaped by the City's focus on revenue rather by public safety needs" and that these practices "disproportionately harm Ferguson's African-American residents."

[7] See "Timeline of events in shooting of Michael Brown in Ferguson," *Associated Press News*, August 9, 2019, https://apnews.com/article/shootings-police-us-news-st-louis-michael-brown-9aa32033692547699a3b61da8fd1fc62, discussion of August 2014 protests in "Michael Brown protests in Ferguson met with rubber bullets and teargas," *The Guardian*, August 14, 2014 https://www.theguardian.com/world/2014/aug/14/ferguson-police-teargas-rubber-bullets-michael-brown, and discussion of August 2015 events in "Ferguson state of emergency ends after days of protests", *CBS News*, August 14, 2015 https://www.cbsnews.com/news/ferguson-missouri-state-of-emergency-ends-michael-brown-anniversary-protests/

Subsequently, the state of Missouri passed several laws and updated court rules in an effort to curb so-called policing for profit. In this paper, we primarily focus on the reform in Missouri Senate Bill 5 (SB5). This reform, effective in 2016, limited local governments from raising more than 20 percent of general revenue from traffic fines and fees. Fines and fees in excess of 20 percent must be remitted to the state government.[8] As we argue below, this leads to sharp changes in incentives for revenue generation, with the local share from the marginal ticket dropping from 100 percent to 0 percent in places treated by the law, those raising more than 20 percent of general revenue from policing at baseline.

In addition, there there were two reforms in response to the events in Ferguson, and we try to control for these reforms in our analysis:

1. Rule 37.04, effective on July 1, 2017, limited the ability of Missouri courts to issue warrants and to hold individuals in jail for minor traffic violations or municipal ordinance violations (https://www.courts.mo.gov/page.jsp?id=115880). These changes made it more difficult for municipalities to collect the fines from issued tickets.[9]

2. A local non-profit, "ArchCity Defenders," filed a series of civil rights lawsuits against municipal governments alleging police misconduct, improper jail conditions, and unlawful use of jail time to enforce fine collection. Numerous settlements associated with these lawsuits included changes in both policing practices and municipal court practices around revenue generation.[10]

4 Data

To examine responses to the shooting in Ferguson as well as the subsequent reforms around policing for profit, we develop measures of both municipal court collections and police stops at the municipality-year level. Court statistics include information on municipal court collections, cases, and warrants, by municipality and state fiscal year 2010-2019.[11] The state fiscal year runs July 1-June 30, meaning that the Ferguson shooting occurred during fiscal year 2015, which began on July 1, 2014, a few months before the shooting. Importantly, this measure of collections

[8] The original S.B. 5 passed on July 5, 2015 further limited municipalities in St. Louis County to 12.5%, rather than the 20% applied to all other municipalities. This was struck down by the Missouri Supreme Court in 2017, making the limit 20% for all municipalities in the state, see https://www.senate.mo.gov/15info/pdf-bill/tat/SB5.pdf) and discussion in Rubinstein (2021).

[9] St. Louis Dispatch April 23, 2018

[10] See discussion at https://finesandfeesjusticecenter.org/articles/post-ferguson-class-action-suits-challenging-fine-and-fee-schemes-in-missouri/.

[11] https://www.courts.mo.gov/page.jsp?id=155973

includes revenue both retained by the municipality in addition to any revenue that would have to be remitted to the state in the event that the municipality was over the 20 percent cap on retained revenue after the implementation of SB5. Thus, this measure captures total collections associated with policing.

Our data on police stops are taken from the "Vehicle Stops Reports" provided publicly by the Office of the Missouri Attorney General and cover every municipal police department over the calendar years 2009-2019. These data include (separately by race) counts of the number of stops, stop location (e.g. interstate), stop outcomes (e.g. warning versus citation), arrests, arrest charge (e.g. outstanding warrant), searches, search outcomes, etc.[12] These data are organized by calendar year and thus the 2014 Ferguson shooting aligns with the data from 2014.

These two key datasets are supplemented with the following data:

1. Municipal government finances from the US Census of Governments data, conducted every five years. We use information on general revenues to measure whether municipalities were raising more than 20 percent of their general revenue from court collections at baseline, which we take to be 2012 in this case.

2. The length of interstate highways, US and State highways, and city roads using GIS data from the MO Department of Transportation.[13] We use these data to develop a measure of interstate presence, a proxy for the motive and opportunity for engaging in policing for profit.

3. Traffic data from the federal Department of Transportation. We use the data for Missouri to develop a measure of highway traffic, a second proxy for the motive and opportunity for engaging in policing for profit.

4. Total population and population by race for each municipality in 2010 using the 2010 US Census data, obtained from the National Historical GIS project.[14]

5 Responses to Ferguson

We begin our analysis by documenting the evolution of our key outcomes via a time-series analysis, using the Ferguson shooting as the event, which in turn triggered the reforms implemented in the state of Missouri. That is, we first consider the Ferguson shooting as the treatment and are

[12] https://ago.mo.gov/get-help/vehicle-stops-report/

[13] Municipal boundaries are available here: https://msdis-archive.missouri.edu/archive/Missouri_Vector_Data/Administrative_Political 2010 roads are available here: https://msdis-archive.missouri.edu/archive/Missouri_Vector_Data/Transportation_Networks/.

[14] Census data can be downloaded from NHGIS here: https://www.nhgis.org/documentation/tabular-data.

agnostic in this section about the specific mechanisms or policies driving any changes, an issue to which we turn to in the next section.

Figure 1 shows that court collections per capita were increasing in Missouri during the years leading up to the Ferguson shooting, starting at around 75 dollars per capita in fiscal year 2010 before peaking at almost 90 dollars per capita in fiscal year 2013. As noted above, the shooting occurred in state fiscal year 2015. Following the shooting, there was a small drop in collections in 2015 and then larger drops in 2016, the first year of the cap on retained revenue (SB5), and 2017, before leveling off at less than 40 dollars per capita starting 2017. Thus, collections fell by more than 50 percent between fiscal year 2014, the fiscal year prior to the shooting, and fiscal year 2019, the end of our sample period.

Turning to police stops per capita, we document similar patterns in Figure 2, with over 0.5 stops per capita during the years leading up to the shooting in Ferguson. However, unlike the timing of the effects for collections, most of the decline in stops occurred in calendar year 2015, immediately following the shooting in Ferguson. After calendar year 2015, stops per capita level off at around 0.4 stops per capita (between 2016 and 2019). Thus, the timing of these responses suggests that post-Ferguson reforms around revenue (e.g. SB5) may play a role independently of any Ferguson effect, defined as a de-policing response for reasons unrelated to revenue. Further, in terms of relative magnitudes, stops per capita fell by roughly 20 percent between 2014 and the end of our sample period, which is less than half the proportional drop in revenue. This suggests that there were changes in revenue-driven policing over and above the potential direct effect of Ferguson on the likelihood of police officers making stops.

We next investigate which municipalities were driving these post-Ferguson declines in collections and police stops. To the extent that these declines in Figure 1 and Figure 2 are driven by the post-Ferguson reforms, which were designed to curb policing for profit, the declines should be driven by places raising significant revenue from policing at baseline. To investigate this issue, we begin by categorizing municipalities into four quartiles based upon their per-capita collections at baseline (2012). As shown in Figure 3, there were significant outliers in collections at baseline, with the top quartile averaging roughly 250 dollars per capita prior to the Ferguson shooting. The other three quartiles, by contrast, all average less than 50 dollars per capita at baseline, illustrating the extreme heterogeneity in reliance on policing in funding local services. Following the shooting in Ferguson, which occurred during fiscal year 2015, there are immediate declines in collections per capita, as shown, in places that were raising the most at baseline. The downward trend continues until fiscal year 2017, at which point collections per capita level off at less than 100 dollars per capita. Thus, collections fall by more than 150 dollars per capita, or by more than half of the baseline levels. While there are also post-Ferguson declines in the other quartiles,

these are significantly smaller in both absolute per-capita terms and in percentage terms.

Turning next to our second outcome, police stops per capita, we observe similar patterns, as shown in Figure 4. Among municipalities collecting the most per capita from policing at baseline (2012), stops per capita are significantly higher at baseline, rising to over 1.5 stops per capita just prior to the 2014 shooting in Ferguson. Stops per capita for this group then fall precipitously, to roughly 1 stop per capita in calendar years 2016-2019. For the other three quartiles, stops per capita are much lower at baseline, averaging less than 0.4 stops per capita. Likewise, there are no apparent declines in stops per capita for these bottom three quartiles following the shooting in Ferguson.

Thus, for both collections and stops, we document that the overall declines in collections and stops post-Ferguson were driven by municipalities that were raising significant amounts at baseline. Of course, this begs the question of why these places were raising significant amounts at baseline. To investigate this issue, we next incorporate measures of the opportunity and motive to engage in policing for profit. As noted above, municipalities with a significant number of non-residents driving through have a particular incentive to engage in policing for profit given the opportunity to export their local government financing to non-residents, who, by definition, do not vote in local elections in the ticketing jurisdiction.

Given this motivation around tax exporting, we first incorporate measures of interstate penetration in the municipality. We begin by documenting a relationship between interstate penetration and collections from policing at baseline. As shown in Figure 5, which aggregates collections from the municipality to the county level at baseline (2012), there is a relationship between the interstate system and collections per-capita. That is, places with interstates passing through their borders tend to collect significantly more on a per-capita basis at baseline. As shown, this appears to be driven by the St. Louis metro area. To study this area in more detail, we plot collections per capita along with the interstate system across municipalities within the St. Louis area in Figure 6. As shown, there is again visual evidence of higher collections in areas with interstates passing through their municipal boundaries.

Based upon this motivation, we next examine whether places with interstate penetration experienced the largest declines in collections and stops following the shooting in Ferguson. We define the interstate ratio as the interstate length in the municipality scaled by square miles. We then run regressions of collections per capita on this measure, allowing the coefficient to vary on a year-by-year basis, with the baseline year defined as 2014 for our collections per-capita measure (state fiscal year) and 2013 for the stops per capita measure (calendar year). All regressions also include a series of two-way fixed effects (municipality and year). As shown in Figure 7, places with a significant interstate presence experienced a decline in collections, relative to areas

with less interstate presence, on a per capita basis following the shooting. While there is a small relative decline in 2015, the fiscal year in which the shooting occurred, there are much larger declines between 2016 and 2019. As shown in Figure 8, there is a similar pattern for stops per capita, which decline by a small amount in calendar year 2014, the year of the Ferguson shooting. The decline accelerates in calendar years 2015 and 2016 and then becomes somewhat weaker in 2017 through 2019. Thus, for both collections and stops, there is a relative post-Ferguson decline in policing in places with significant interstate presence, which provide both an opportunity and a motive to engage in policing for profit.

Our second measure of the opportunity and motive for policing for profit involves traffic. In particular, we develop a measure of average annual daily traffic (AADT) on highways in each municipality at baseline using the traffic data described above.[15] While not all traffic involves non-residents, our focus on highway traffic likely captures a significant number of non-residents via drivers passing through municipalities on highways as part of a longer trip. As shown in Figure 9, we document a pattern similar to that in Figure 7, which is based upon interstate penetration. In particular, places with significant highway traffic, relative to places with less highway traffic, experience an immediate decline in collections per capita in 2015, the fiscal year in which the shooting occurred. There is a further decline in fiscal year 2016 before the effect levels off during the 2017-2019 period. While the effects for stops per-capita are less pronounced, as shown in Figure 10, there is evidence of a post-Ferguson decline in stops in calendar years 2015 and 2016 for places with significant levels of highway traffic at baseline, relative to places with less highway traffic. Overall, for both collections and stops, there is again evidence of post-Ferguson declines in policing for places with significant highway traffic, providing both an opportunity and a motive to engage in policing for profit.

To summarize, our results thus far document a significant decrease in collections and stops post-Ferguson. These declines are driven by places raising significant amounts from policing at baseline. Moreover, we show that the post-Ferguson declines are stronger in places with interstate presence and significant traffic, both of which provide an opportunity and a motive to engage in policing for profit.

6 Responses to the Cap on Retained Revenue

While we have documented post-Ferguson declines in policing in places that were raising significant amounts at baseline and with the opportunity and motive to engage in policing for profit, the

[15] In particular, we compute AADT for each segment of highway in each municipality and then, to avoid double counting vehicles, take the maximum AADT across all segments within a municipality.

analysis thus far does not identify specific mechanisms. We now turn to assessing the effect of Senate Bill 5. To identify the causal effect of this reform, we exploit both the timing of this policy and cross-sectional variation in treatment intensity, using a difference-in-difference framework.

Most importantly, SB5 limited local revenue from policing to 20 percent of general revenue, with any revenue collected above this threshold remitted to the state. This implies that when a municipality crosses this revenue threshold, the local share from a marginal dollar of policing falls from 100 percent to zero. Above this threshold, a municipality with pure revenue motives should respond by reducing policing, while a municipality with pure safety motives should arguably not adjust their policing behavior. Thus, this analysis of the limit on retained revenues allows us to shed light on the underlying objective of police departments.

Empirically, using measures of total collections and general revenue from baseline (2012), we create an indicator for SB5 treatment equal to one for municipalities collecting more than 20 percent of their revenue from fines and fees at baseline. For the numerator, we use the court collections data, and, for the denominator, we use general revenues from the Census of Governments. Both are measured during the baseline year 2012. According to this measure, we have 45 treated municipalities and 399 untreated municipalities. To examine the robustness of this measure of treatment, we also develop alternative measures of treatment below.

We first estimate event studies comparing treated and untreated municipalities before and after the Ferguson shooting and the subsequent implementation of SB5 during 2016. We include two-way fixed effects (municipality and year) and cluster standard errors by municipality. As shown in Figure 11, which focuses on collections, there is a small decline in places treated by SB5 in fiscal year 2015—before the implementation of SB5—relative to control places. However, the decline becomes much more pronounced in fiscal year 2016, the start of the reform. Overall, revenue from policing in treated places declines on average more than 100 dollars per capita between 2016 and 2019, relative to control municipalities.

To better illustrate the magnitude of these effects, we use the estimates from Figure 11 to create a simple counterfactual plot showing the dynamics of collections, with and without SB5 and with and without a simple measure of post-Ferguson scrutiny. In Figure 12 the black line ("Raw data") shows the change in collections per capita observed in the data, similar to Figure 1, but restricted to our estimation sample. The red line ("No SB5") shows a counterfactual without SB5, which we implement by setting the SB5 by year effects to zero, for years 2016-2019. Finally, we approximate the Ferguson effect as the drop in collections that occurred in 2015, before SB5 was implemented. To do so, we subtract the SB5-by-2015 coefficient shown in Figure 11 from post-2014 predicted revenue, while retaining the 2016-2019 SB5-by-year coefficients. This is shown by the gray line ("No Ferguson"), where the vertical distance between the "No

SB5" and "No Ferguson" lines starting in 2016 shows the difference in magnitude between the 2015 coefficients and 2016-2019 coefficients. As shown, the magnitude of the SB5 effect on per capita collections is large, even after allowing for a permanent post-Ferguson drop from scrutiny. This suggests that our results are driven by the sharp change in incentives associated with limiting the local share of the budget from policing to 20 percent.

While we have documented declines in revenue and have linked these to the cap on retained revenue, these results do not confirm that municipalities were complying with the cap. It could be, for example, that municipalities dropped their share of revenue from policing from 40 percent to 30 percent and were thus above the 20 percent cap. To shed light on this issue, we next incorporate data on general revenues from the 2017 Census of Governments. Using this information, we compare the 2012 share of revenue from policing, the measure that we used to define treatment status, to the corresponding 2017 share. As shown in Figure 13, by construction, all treated municipalities were raising more than 20 percent of revenue from policing and all control municipalities were raising less than 20 percent of revenue from policing. By 2017, by contrast, roughly 75 percent of treated municipalities were raising less than 20 percent of revenue from policing. For control municipalities, we find that nearly all of them continue to raise less than 20 percent of revenue from policing in 2017.

Turning to our data on stops, we estimate corresponding event studies, As shown in Figure 14, there is no decline in 2014, the year of the shooting, but a large decline in 2015 and an additional decline in 2016, the calendar year in which SB5 became effective. Overall, there is a decline of roughly 0.5 stops per capita in places treated by SB5, relative to control municipalities. Combined with our results on collections, our findings suggest that municipalities responded to the change in incentives induced by SB5 via reductions in both policing and corresponding revenue collection.

As a companion to these event studies and to summarize our treatment effects in a simple way, we next provide results from simple two-way fixed effects regressions, In Table 1 we show the coefficients on the interaction between a SB5 treated and Post, where SB5 treatment is defined as above and Post is defined as fiscal year 2016 or later for collections, and calendar year 2016 or later for stops. As shown in Column 1, we find that collections per capita decline by $143.5. Column 1 of Table 2 reports similar results for stops. As shown, stops per capita decline by 0.39 in treated municipalities, relative to untreated municipalities, after the implementation of the SB5 reform. Taken together, these results are consistent with our evidence from event studies, which document economically and statistically significant reductions in both collections and policing in treated places following the implementation of SB5. This evidence is again consistent with revenue-driven municipalities reducing their policing efforts in response to the sharp changes in incentives brought about by the reduction in the local share from a marginal ticket in treated

municipalities.

6.1 SB5 versus post-Ferguson scrutiny

Our evidence on SB5, which documents a differential post-Ferguson change for municipalities treated by SB5, is consistent with a change in the incentives for revenue-driven policing. However, it is possible that SB5 treatment may be correlated with other municipality characteristics that would also predict a decline in the presence of increased scrutiny following the shooting, such as interstate highway access. That is, it is possible that areas that were treated by SB5 also had disproportionate scrutiny, leading to a reduction in policing effort and associated collections for reasons that were not driven by the SB5-related change in revenue incentives. We attempt to address this issue in four ways.

First, while our baseline analysis involves comparisons across all municipalities in Missouri, we narrow the focus to comparisons within areas of Missouri that arguably had more similar post-Ferguson scrutiny. We begin by limiting the sample to municipalities within St. Louis County and the neighboring City of St. Louis. Given that Ferguson is located within St. Louis County, we expect scrutiny to be highest in this region.[16] As shown in column 2 of Table 3 (Panel A), the estimated effects in St. Louis are similar to our main estimates in column 1 for both stops and collections. Likewise, we next restrict the sample to municipalities outside of St. Louis. These municipalities are both disproportionately white and geographically far from Ferguson; given this, we expect them to have arguably uniformly lower levels of scrutiny of policing post-Ferguson. As shown in column 3, we continue to find large and statistically significant effects for both collections and stops. We next turn to places with interstate highways, which, as argued above, had the opportunity and motive to engage in policing for profit and were thus arguably more likely to experience increased scrutiny following Ferguson. As shown in column 4 of Table 3, municipalities both treated by SB5 and with interstates experience a decrease in per capita collections of $184, compared to municipalities that are both untreated and have interstates. The effect on stops per capita is statistically insignificant, potentially reflecting the reduced sample size, but roughly comparable in magnitude to our main estimate in column 1. Lastly, we examine only municipalities in the top quartile of fines per capita in 2012, based again on the idea that they might be disproportionately subject to increased scrutiny following Ferguson due to their significant revenue generation at baseline. As shown in in column 5, our estimates, which compare places raising significant amounts at baseline but with different exposure to SB5, are close to our baseline results in column 1. Overall, these results document that, even within

[16] We drop Ferguson from all of our estimation samples since this city was subject to a consent decree, which could affect outcomes independent of SB5.

subsets of municipalities likely to face similar levels of post-Ferguson scrutiny, we find large and statistically significant treatment effects from SB5 that are comparable to our baseline estimates.

Related to this idea of comparing places with similar levels of scrutiny, we next run our two-way fixed effects analysis for the entire sample but introduce controls designed to proxy for post-Ferguson scrutiny. These results are presented in Panel B of Table 3. In the second column, we add a time-varying controls for the St. Louis metro area. In particular, we include an interaction between the year fixed effects and an indicator for whether the municipality is within St. Louis County (or the City of St. Louis). While this specification allows for the possibility of de-policing resulting from greater post-Ferguson scrutiny in these areas, the coefficients are similar to baseline our baseline results in column 1. We next introduce flexible controls for the racial composition of the municipality. In particular, we interact year fixed effects with the Black population share of the municipality, as measured in the 2010 Census. This specification allows for post-Ferguson scrutiny to vary with the racial composition of a municipality, capturing the racial salience around any enhanced post-Ferguson scrutiny of police departments. As shown in column 3, the results are similar to our baseline results in column 1. We next control for two other policy reforms, as described more fully above, that were arguably part of the post-Ferguson scrutiny of police. The first reform limited the ability of municipalities to arrest drivers with outstanding warrants due to unpaid tickets. To address this, we control for the number of warrants issued per capita in 2012, interacted with the year fixed effects, to allow for municipalities issuing more warrants at baseline to follow different trends.[17] As shown in column 4, adding this control does reduce the magnitude of the coefficient for collections; but the effect on collections remains large and statistically significant. By contrast, this control has little effect on our stops estimate. Lastly, in column 5 we control for whether a municipality was ever sued by Arch City Defenders during the post-Ferguson period. To do so, we interact an indicator for ever being exposed to a lawsuit with time fixed effects. As shown in column 5, including this control has little effect on our key coefficients.

Second, we note that the dynamics of the treatment effects around collections are consistent with a response to the sharp change in incentives following the implementation of SB5. As documented in Figure 11 above, collections fell only slightly in in 2015, the year following the Ferguson shooting, but the decline accelerated in 2016, the year in which SB5 was introduced. This idea is also captured in Figure 12, which documents that the drop in collections in 2016 explains only a small part of the subsequent declines in collections following the implementation of SB5. Were our results driven by differential post-Ferguson scrutiny in treated municipalities,

[17]This control does not test for the effect of the warrants reform, Rule 37.04, which wasn't implemented until fiscal year 2018, the second to last year of our panel. However, the DOJ report suggested that warrants were routinely issued to ensure fines were paid, and thus this measure could also be correlated with increased scrutiny.

we would have expected larger relative declines in collections in 2015, the year following the Ferguson shooting.

Third, we document that the post-SB5 drop in collections is larger in magnitude than the corresponding post-SB5 decline in stops. If police departments simply scaled back all activity after the shooting in response to increased scrutiny, then we would expect a proportional decline in both revenue and stops. That is, if police officers make, say, 10 percent fewer stops, then mechanically there should be 10 percent decline in revenues. As shown in the first column of Table 4, which reports results in natural logs in order to make the treatment effects comparable across these two outcomes (collections and stops, which are in different units), collections decline by 67 percent, while stops only decline by 18 percent. Thus, these results suggest that any reductions in stops resulting from post-Ferguson scrutiny are not the primary drivers of our post-SB5 decline in revenue.

Building upon this idea, we next develop explicit measures of how much of the decline in collections can be attributed to the post-Ferguson decline in stops. To do so, we first run a two-way fixed effects regression of fiscal year collections per-capita on stops per-capita during the prior calendar year using only the pre-Ferguson data.[18] This yields a regression coefficient of roughly 35 dollars, with a standard error of 10. Thus, and not surprisingly, there is a within-municipality positive correlation between collections and stops, with an additional stop generating roughly 35 dollars in collections. Based upon this regression, we then predict collections per capita using only information on stops per capita, and we do so for all years, both during the pre-Ferguson and post-Ferguson periods. Finally, we run an event study using this measure of predicted collections per capita as the dependent variable. As shown in Figure 15, we continue to find post-SB5 declines in collections per capita, as predicted by stops. Yet the magnitude of the effect, which documents a decline in predicted collections per capita of roughly 15-20 dollars, is well below our baseline reduction of roughly 150 dollars. Thus, the decline in collections predicted by the post-SB5 decline in stops explains only a small fraction of our estimated decline in collections attributed to SB5, with the remainder due to a reduction in collections per stop.

Fourth, we examine the distinction between stops resulting in citations, which generate revenue, and stops results in warnings, which do not generate revenue. To the extent that our baseline results are driven by revenue incentives associated with the change in incentives brought about by SB5, we would expect our results to be driven by citations rather than warnings. As shown in columns 3 and 4 of Table 4, this is indeed the case: while the treatment effect for citations per capita is similar to that in our baseline results, the treatment effect for warnings per capita, by

[18] We use lagged stops for two reasons. First, the fiscal year ends before the calendar year, and, second, stops can generate future collections but not vice versa.

contrast, is small and statistically insignificant. This is true despite the fact that roughly 40 percent of municipal stops in Missouri resulted in warnings during the pre-Ferguson period. Thus, the fact that our results are driven by citations rather than warnings is consistent with a response to the sharp change in revenue incentives induced by SB5.

6.2 Additional Analysis

We next attempt to answer the following question: Is the reduction in collections and stops driven by a reduction in policing effort holding fixed resources, in terms of size of the police force and/or the budgets for the police department? Or is the reduction in collections and stops driven, at least in part, by a reduction in resources? Police departments treated by SB5 could experience a reduction in resources for at least two reasons. First, municipalities with pure revenue motives around policing might face incentives to scale back resources in the presence of a cap on retained revenues. And, second, municipalities might have experienced a negative fiscal shock associated with SB5, leading to an overall reduction in government spending and payroll. To answer this question, we incorporate data on the size of the police force and policing budgets using data from the Census of State and Local Law Enforcement. Overall, as detailed in Online Appendix Section 9.2, we find no evidence that police departments treated by SB5 reduced the size of their police force or experienced reductions in their budgets. Thus, the reduction in collections and stops documented in this paper appear to be driven by a reduction in policing effort holding resources fixed.

Returning to our baseline estimates, we next run a series of robustness checks. We begin with two alternative measures of SB5 treatment. We first develop a continuous measure of treatment defined as the baseline share in excess of the threshold (2012 collections share minus 20 percent, for those municipalities above 20 percent, and zero otherwise). This measure accounts for the fact that places raising larger shares at baseline may be treated more intensely. As shown in column 2 of Tables 1 and 2, the results for both collections and stops are similar using this measure. In the second robustness check, we develop an alternative measure of general revenue, which is used in the denominator when measuring the fraction of the municipal budget funded by policing at baseline. In particular, we use a measure of general revenue obtained directly from a municipality's Certified Annual Financial Reports (CAFRs). While we only have data from CAFRs for municipalities in St. Louis County in 2012. we create a measure of predicted revenue share by regressing the 2012 share of revenue from fines in St. Louis County municipalities (using CAFRs data for the denominator) on 2012 fines per capita, which yields a fairly close fit ($R^2 = 0.74$). Based upon this regression, we then predict the fraction raised from policing for all municipalities in Missouri using only variation in fines per capita. Finally, we classify

municipalities as treated if this predicted fraction is greater than 20 percent. As shown in column 3 of Tables 1 and 2, we our estimates based upon this alternative measure are similar to those in our baseline specification.

We next include additional and more flexible geographic controls. While we previously introduced St. Louis-by-year fixed effects, we next include a more general specification with county-by-year fixed effects. This analysis thus compares municipalities within the same county and same year, with some treated by SB5 and others in the control group. As shown in column 4 of Tables 1 and 2, the results with these more flexible geographic controls are similar to those in our baseline specification. Finally, we drop very small cities, those with few than 500 residents, as measured in the 2010 Census. Given our use of per-capita measures, the inclusion of small municipalities can lead to significant outliers. As shown in column 5 of Tables 1 and 2, the results when excluding these very small municipalities are similar to those in our baseline specification.

7 Policing for Profit and Race

Much of the public conversation around policing for profit, and specifically the public discussion in Missouri after the shooting of Michael Brown, focused on racial implications. For example, the DOJ investigative report cited anecdotal evidence of racist statements by local police and city officials in support of it's claim that revenue incentives were a contributing factor to racial disparities in Ferguson (DOJ, 2015). Yet, there is second, less widely discussed, reason that revenue incentives can contribute to racial disparity even in the absence of explicit racial motives. If black communities are disproportionately the target of police action at baseline (e.g. "over-policed"), then any policy that changes the overall level of police activity will *mechanically* have racial implications even if the policy and its implementation are race-neutral. Put slightly differently, policies that incentivize policing for profit have stark implications for racial equity simply because of racial differences in the base rate of policing.

Motivated by the DOJ report and ensuing public discussion, we next consider the racial implications of our results. We begin by asking whether one group benefited disproportionately from the change in revenue incentives wrought by SB5. Since only 11 percent of Missouri residents are black, we explore this possibility by estimating our baseline event study model using the natural log of black stops and white stops as the outcome, making our treatment effects comparable in magnitude across racial groups. As shown in columns 5 and 6 of Table 4, we find a 27 percent reduction in stops for both black and white drivers, consistent with a "color-blind" reduction in overall effort across both groups. This finding is inconsistent with officers differentially reducing their activity for one group more than the other in response to the decline in revenue incentives.

While this reduction in stops was proportional in nature across races, SB5 *did* indeed have implications for the racial incidence of policing. This is due to two important factors. First, black people were more likely to be stopped and cited by police at baseline, prior to Ferguson and the implementation of SB5. More specifically, while black people represented only 11 percent of the Missouri population, they represented 17 percent of all stops. Thus, a proportional reduction in stops that is equal across races will have a larger impact on police stops experienced by black drivers. Second, the municipalities that were treated by SB5 tended to have a higher share of black residents, relative to places not treated by SB5. That is and as shown in Table A1, the black share in treated municipalities averages 28 percent versus a much lower black share (8 percent) in places not treated by SB5. Given this, a disproportionate reduction in stops induced by SB5 in treated municipalities will again have a larger impact on stops experienced by black drivers.

One way to quantify the racial implications of SB5 is to compute how it changed the incidence of police activity by race across the entire state of Missouri. As shown in Table A2, we begin by distinguishing between four types of stops at baseline 1) stops of white drivers in control municipalities, 2) stops of white drivers in treated municipalities, 3) stops of black drivers in control municipalities, and 4) stops of black drivers in treated municipalities. Combining categories 1) and 2) and dividing by the number of white residents of Missouri implies that, on average, whites experienced 0.1514 annual stops per capita by these police departments prior to SB5, while blacks experienced 0.3414 annual stops per capita, more than double the rate for whites.[19] Applying the 27 percent reduction in stops to treated municipalities (categories 2 and 4 above), we then have an estimate of what stop rates would have been had SB5 been in place prior to 2014. Recalculating stops by race, we have that, including the effects of SB5, whites experienced 0.1491 stops while blacks experienced 0.3450 stops per capita. Thus, the reduction in stops experienced black drivers is 0.0164, but only 0.0023 for white drivers, as illustrated in Figure 16. Thus, the reduction in stops experienced by black drivers is more than seven times larger than the corresponding reduction for white drivers. Said differently, there is a reduction of one stop for every 435 white drivers but a reduction of one stop for every 61 black drivers. This is again driven by two factors, the fact that blacks were stopped at higher rates at baseline and the fact that places engaging in policing for profit and thus treated by SB5 had higher black population shares.

Another way to quantify the racial implications of SB5 is to ask how people in municipalities that were targeted by the policy experienced the decline. That is, we ask how stops of drivers in municipalities treated by SB5 were affected, separately by race. To do so, we require a measure of the rate at which black and white drivers were stopped by police in these places at baseline. While

[19] For the baseline, we take an average across the four years prior to Ferguson (2010-2013).

the traffic stops data include stops separately for blacks and white drivers, we lack information on the denominator required to calculate stop rates, namely the share of black drivers. While one can use the racial distribution of residents as a proxy, these statistics suggest that, if anything, whites were policed more heavily than blacks in many places treated by SB5. This can be seen in Figure A1, which documents that places with a large share of black residents, which are more likely to be treated by SB5, have a share of black stops that is lower than their share of black residents (i.e. below the 45-degree line). This pattern is presumably driven by the fact that many stops of white drivers involve non-residents. In the Online Appendix, we develop a statistical approach that, using variation across municipalities in the share of black stops and the share of black residents, allows us to adjust the population to account for non-resident drivers.[20] Based upon this and using this adjusted measure of the racial composition of the municipality, we infer that, as shown in Table A4, black drivers experienced 1.2676 stops per capita at baseline, while white drivers experienced 1.0376 stops per capita. After the 27 percent reduction induced by SB5, the corresponding figures are 0.9253 for black drivers and 0.7574 for white drivers. Thus, as illustrated in Figure 17, black drivers experienced 0.3422 fewer stops per capita, or roughly one stop for every three black residents, while white drivers experienced 0.2801 fewer stops, or roughly one stop for every four white drivers. While the ratio of these two reductions (1.22) is smaller than the ratio using the statewide approach, the decline is still substantially larger for black residents than white. Moreover, the magnitude of the declines for each group is substantially larger in *levels* – highlighting the enormous impact this policy had on people in the municipalities it was intended to target.

In summary, given the initial racial disparity in stops and the fact that treated municipalities had a larger black share, a proportional drop in stops across races led to a disproportionate reduction in stops of blacks, both from a statewide perspective and from the perspective of drivers in treated municipalities.

8 Conclusion

Our results document a clear and consistent post-Ferguson decline in collections and stops. This is driven by places raising significant amounts of revenue from policing at baseline and with the opportunity and motive to engage in policing for profit, as captured by both interstate presence and highway traffic. We then document treatment effects of SB5, which limited retained revenues from policing to 20 percent of general revenue, on policing for profit, with significant reductions in collections and stops. We argue our results cannot be explained by any post-Ferguson increase

[20] The corresponding figure based upon adjusted population can be seen in Figure A2.

in scrutiny of police. This pattern suggests that these policies implemented post-Ferguson may have had the intended effect of decreasing policing for profit. The results are also consistent with a revenue-driven model of policing and inconsistent with a safety-driven model. Taken together, our findings highlight how revenue motives can distort the criminal justice system. From a policy perspective, our results also demonstrate that simple reforms that realign incentives can change police behavior.

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Figure 1: Collections over time

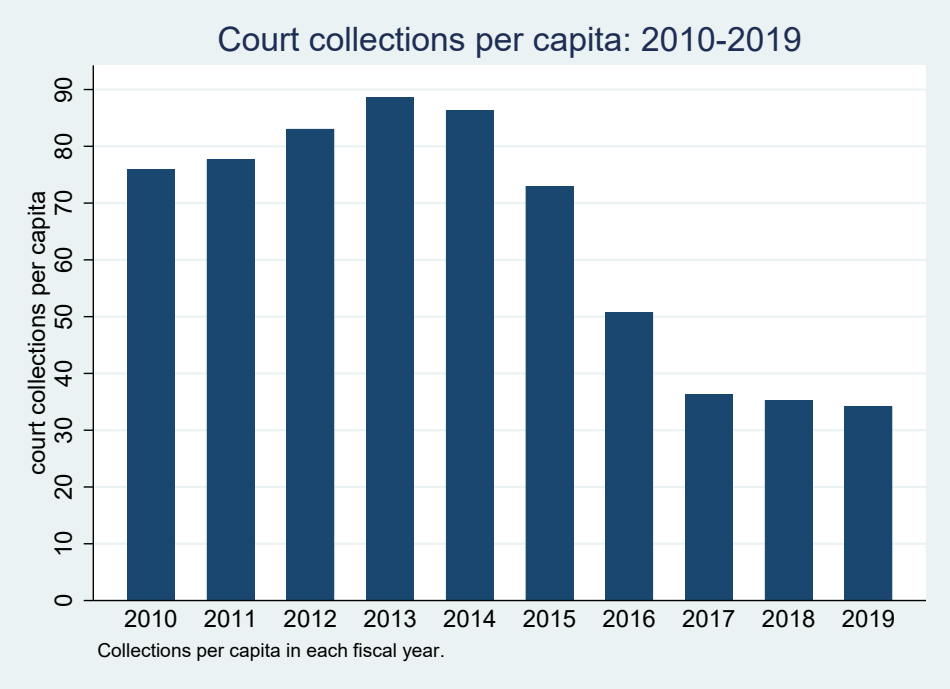


Figure 2: Stops over time

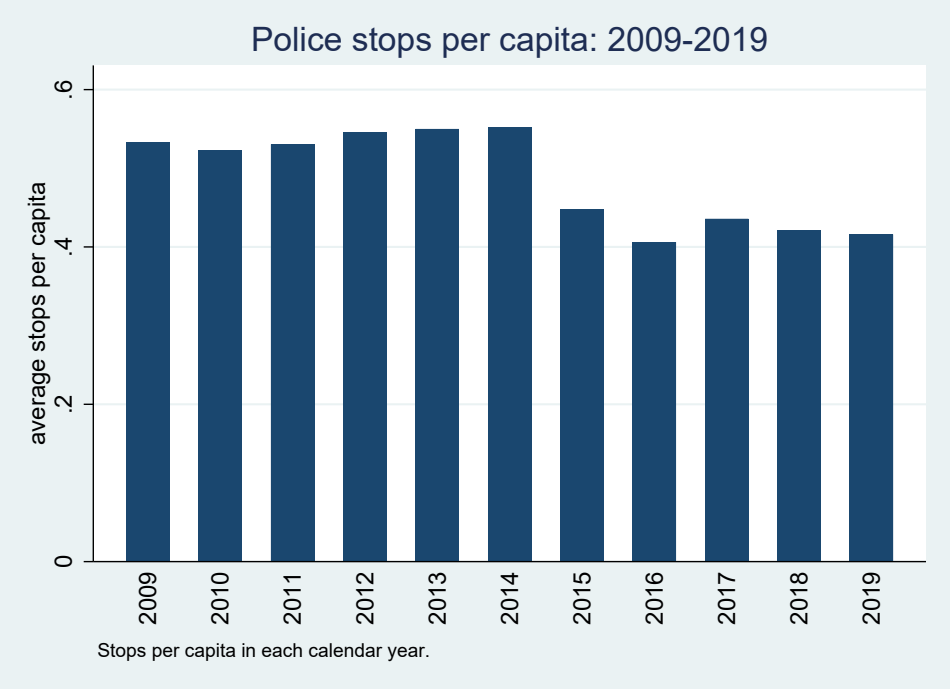


Figure 3: Collections over time by quartile

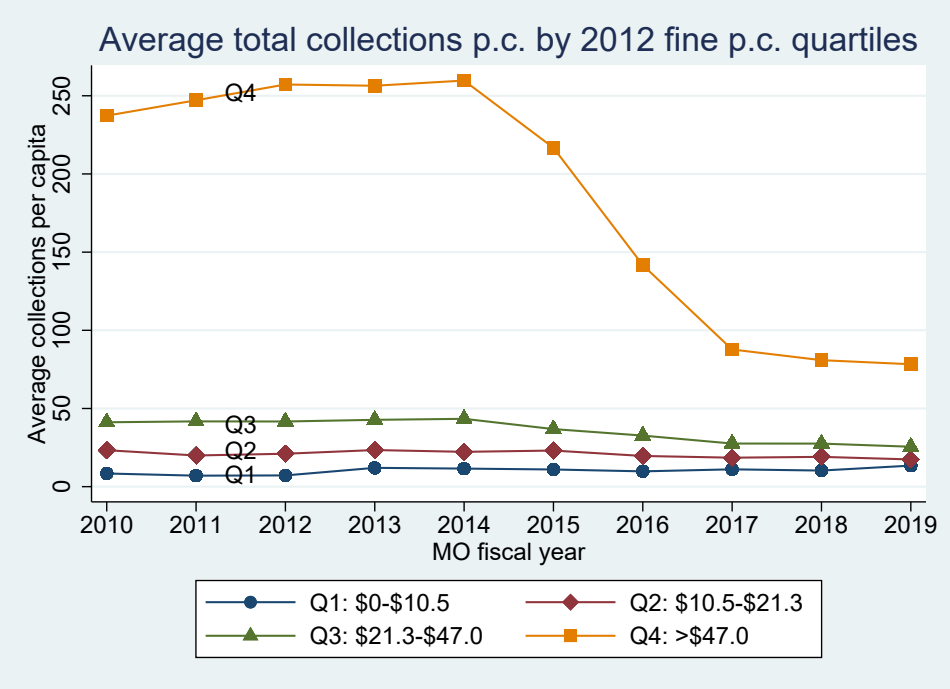


Figure 4: Stops over time by quartile

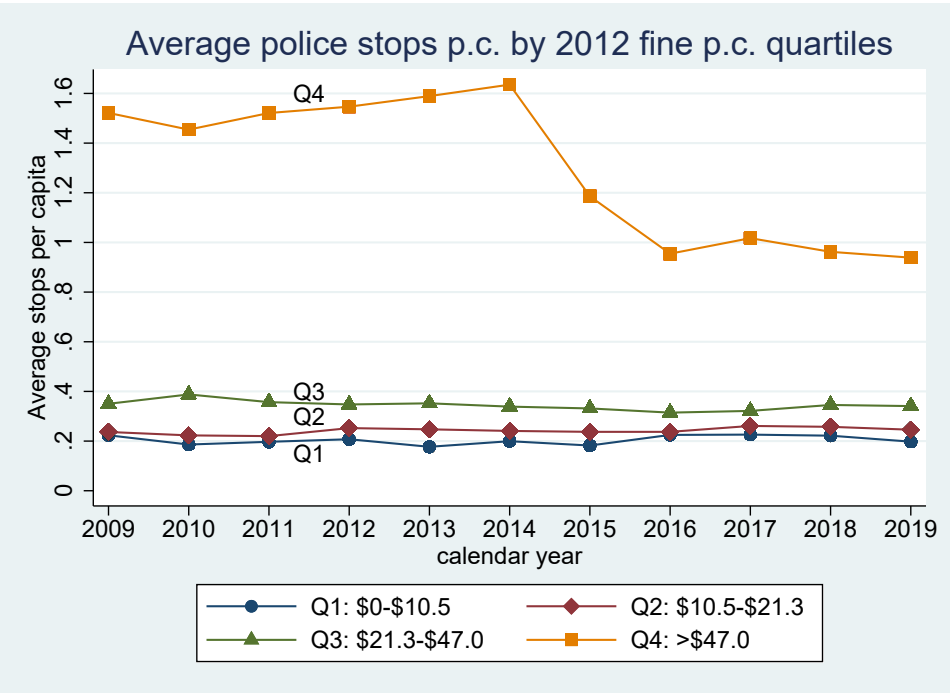


Figure 5: Statewide map

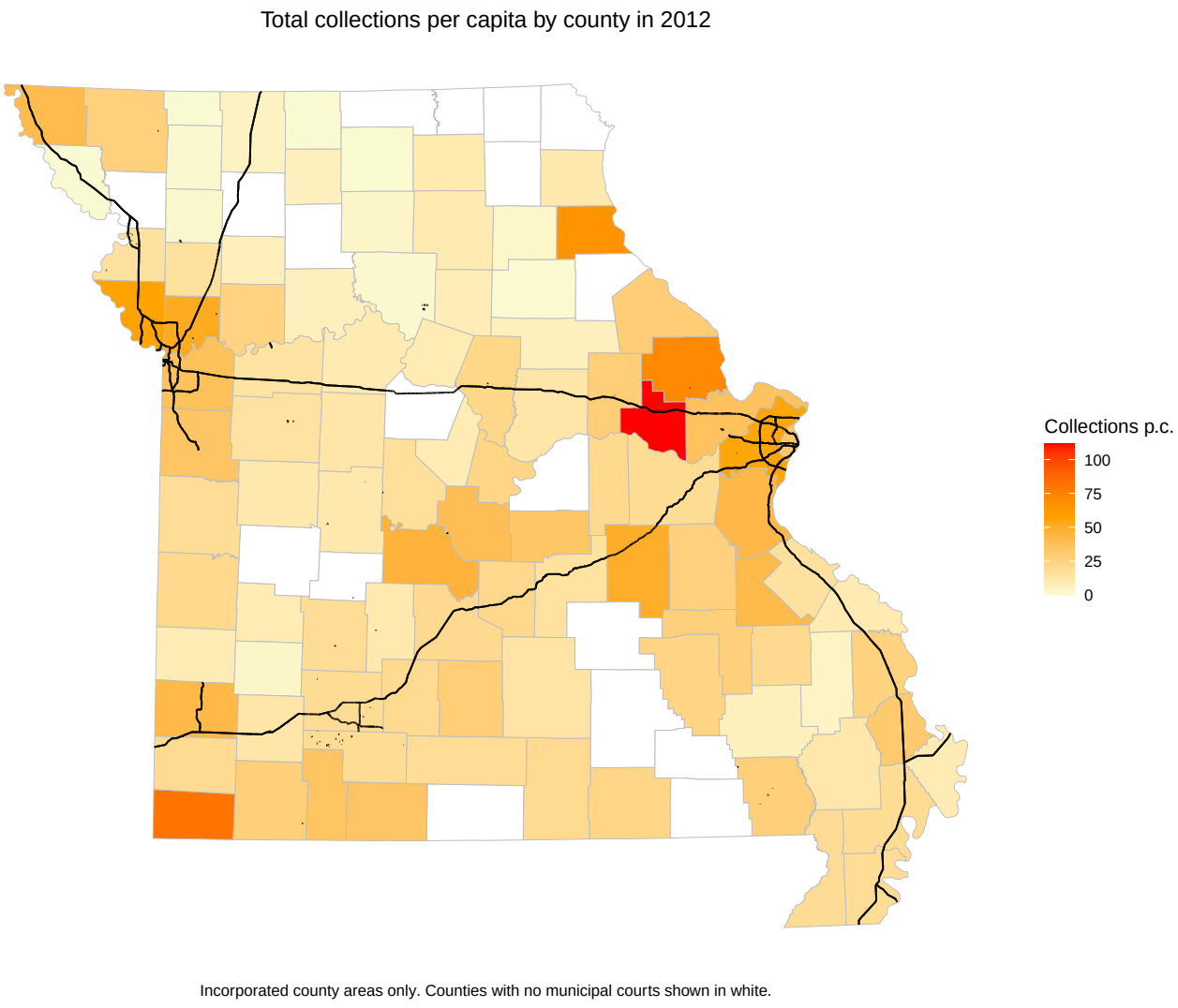


Figure 6: St Louis map

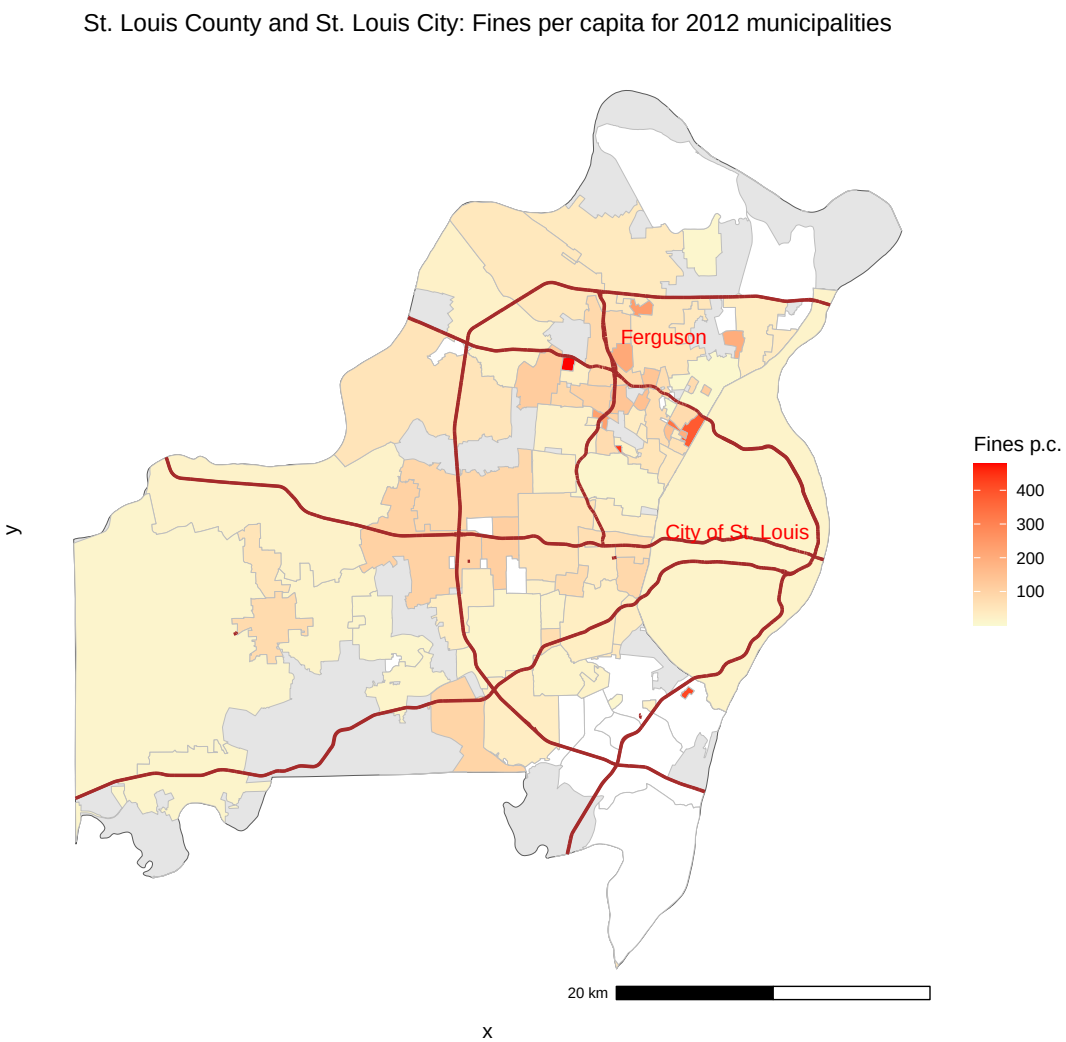
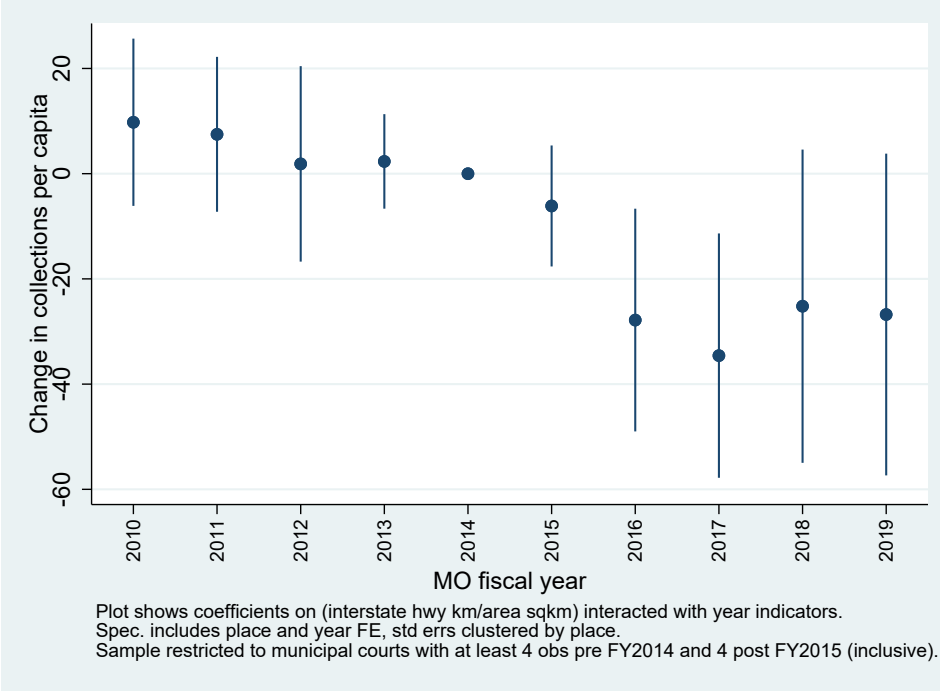
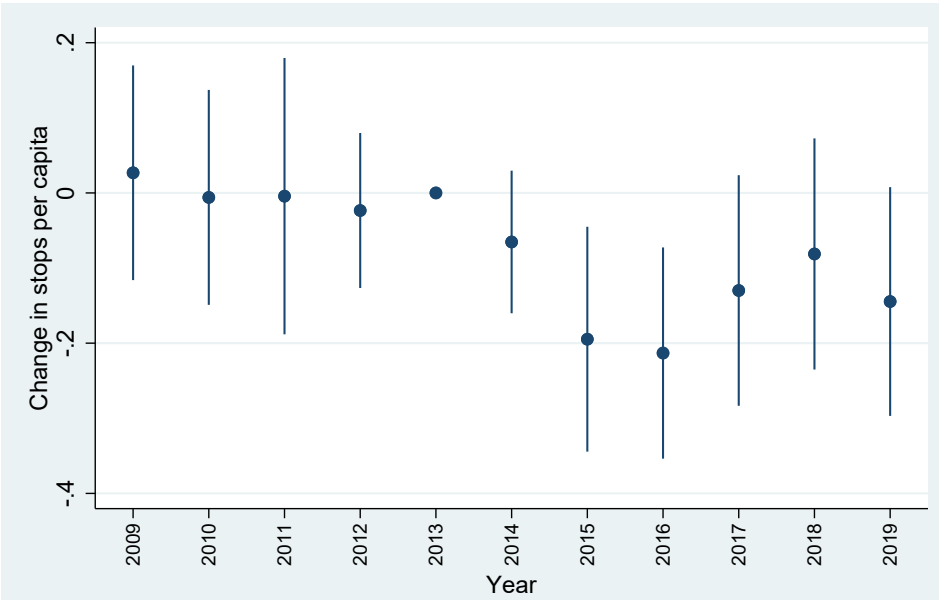


Figure 7: Collections interstate event study



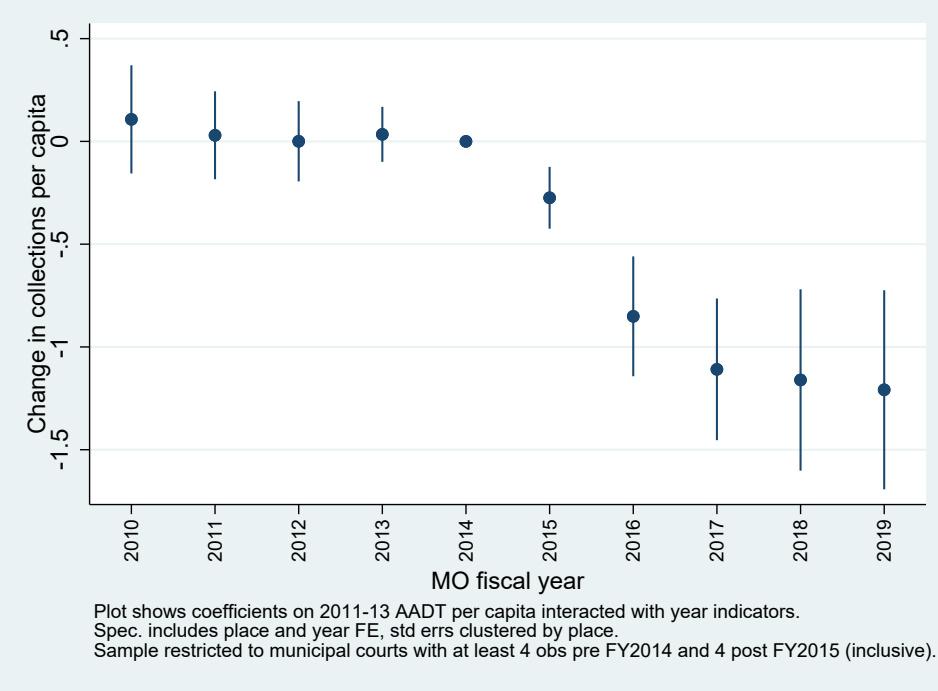
Plot shows coefficients on (interstate hwy km/area sqkm) interacted with year indicators.
Spec. includes place and year FE, std errs clustered by place.
Sample restricted to municipal courts with at least 4 obs pre FY2014 and 4 post FY2015 (inclusive).

Figure 8: Stops interstate event study



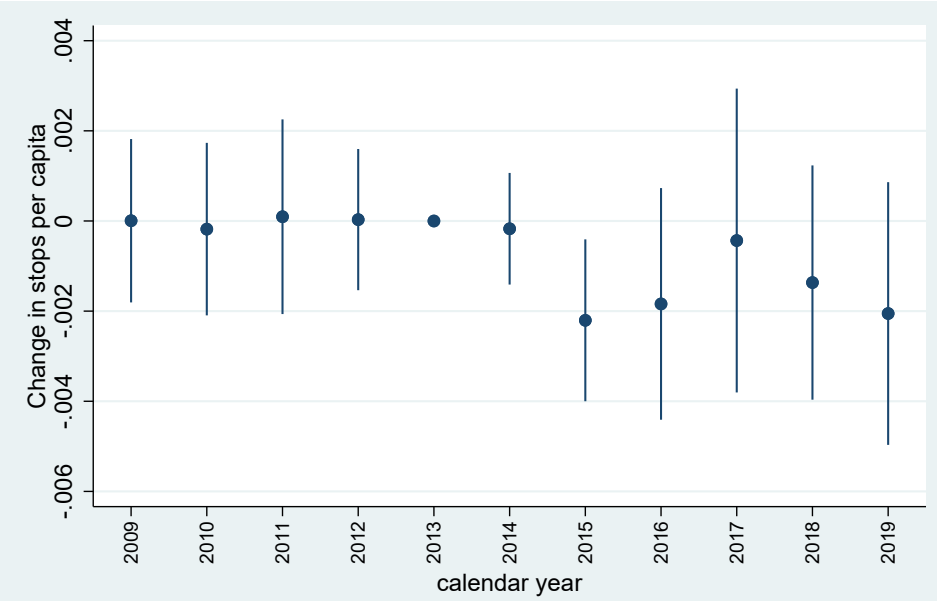
Plot shows coefficients on (interstate hwy km/area sqkm) interacted with year indicators.
Spec. includes place and year FE, std errs clustered by place.
Sample restricted to municipal courts with at least 4 obs pre 2014 and 4 post 2015 (inclusive).

Figure 9: Collections highway traffic event study



Plot shows coefficients on 2011-13 AADT per capita interacted with year indicators.
Spec. includes place and year FE, std errs clustered by place.
Sample restricted to municipal courts with at least 4 obs pre FY2014 and 4 post FY2015 (inclusive).

Figure 10: Stops highway traffic event study



Plot shows coefficients on 2011-13 AADT per capita interacted with year indicators.
Spec. includes place and year FE, std errs clustered by place.
Sample restricted to municipal courts with at least 4 obs pre 2013 and 4 post 2014 (inclusive).

Figure 11: Collections SB5 event study

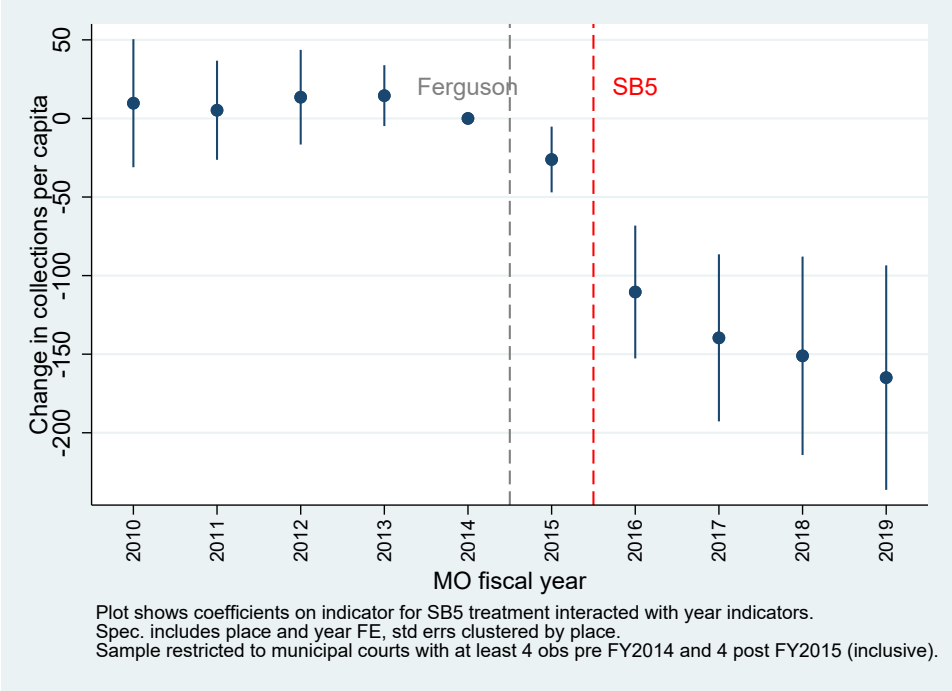


Figure 12: Ferguson effect versus SB5

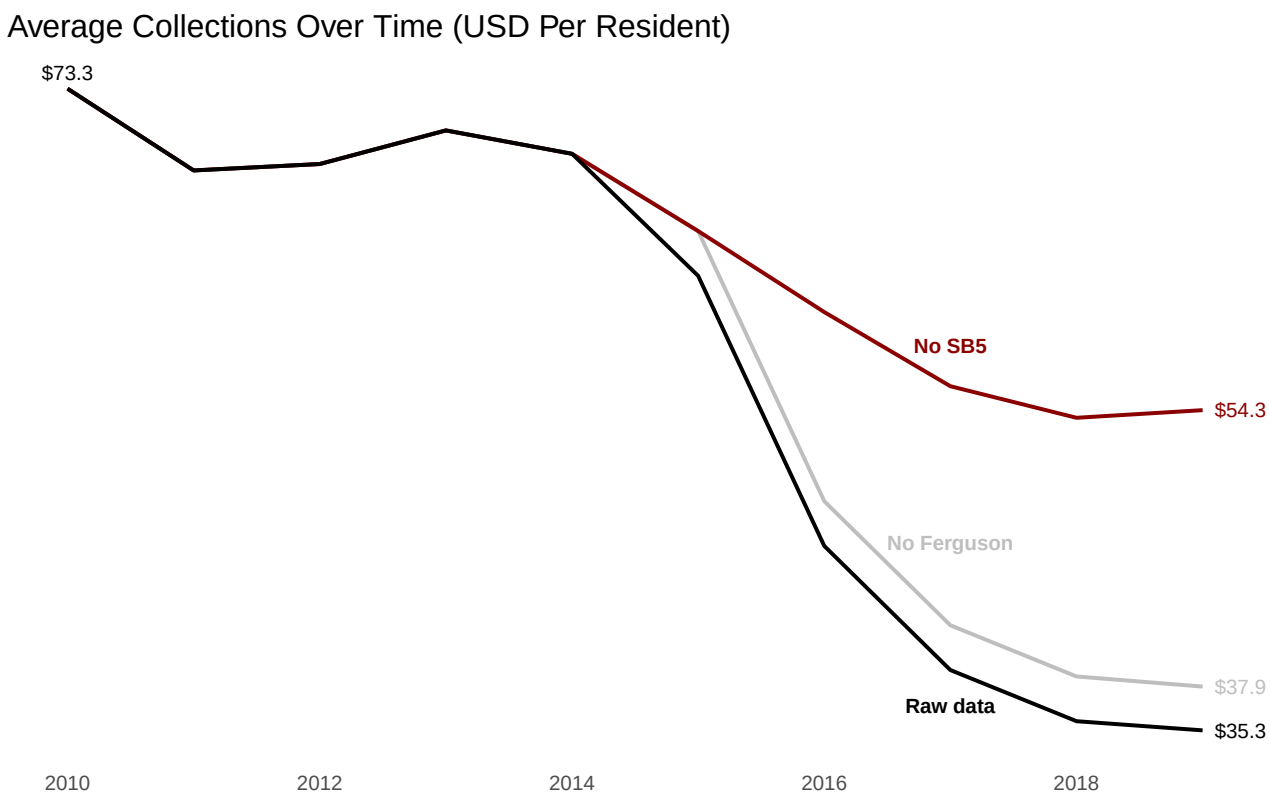


Figure 13: Fraction of collections from policing

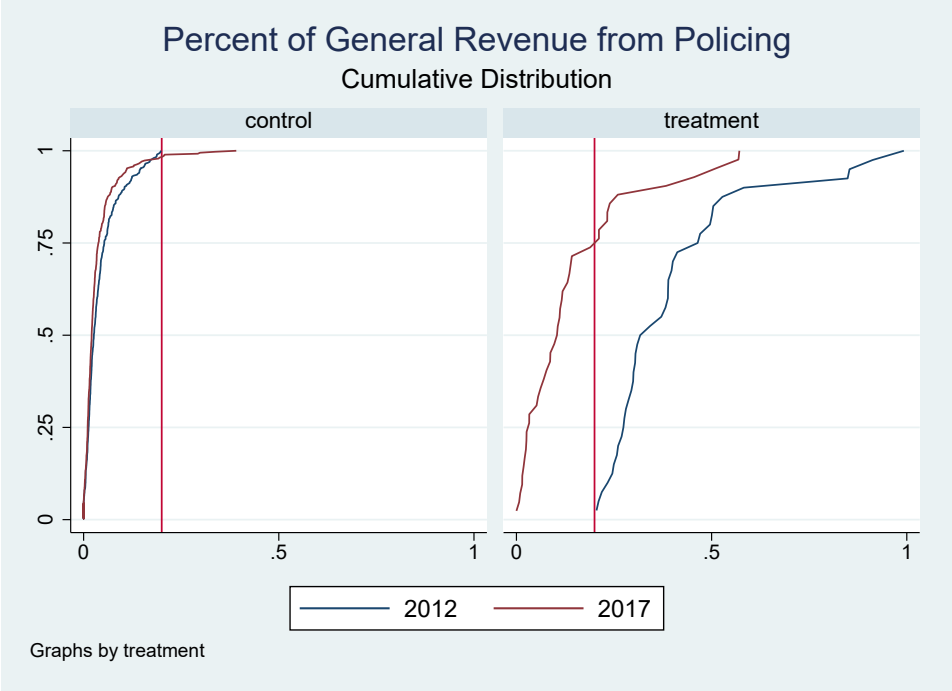


Figure 14: Stops SB5 event study

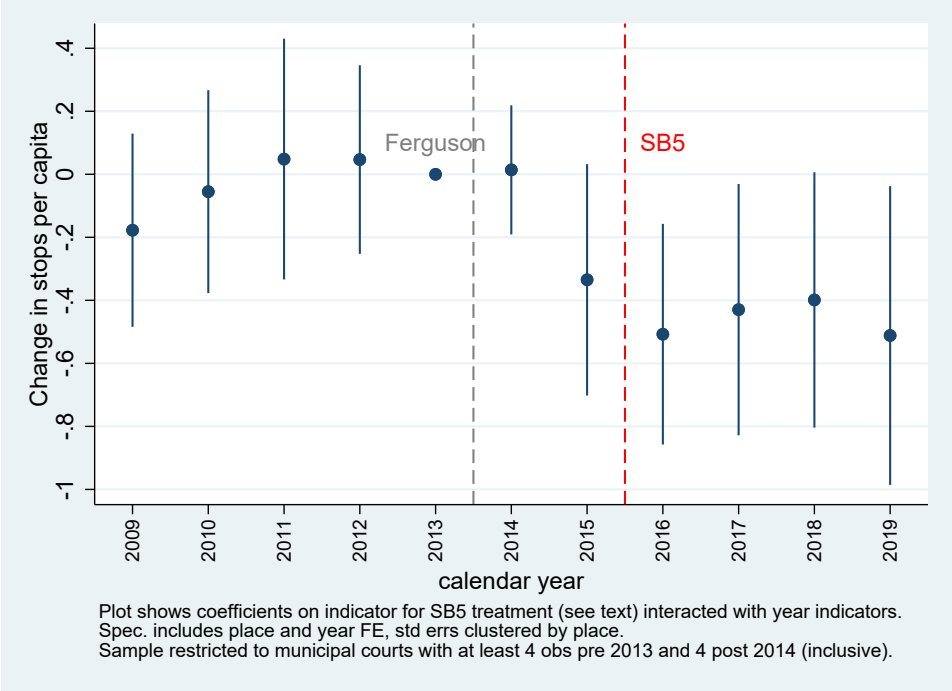


Figure 15: Ferguson effect versus SB5

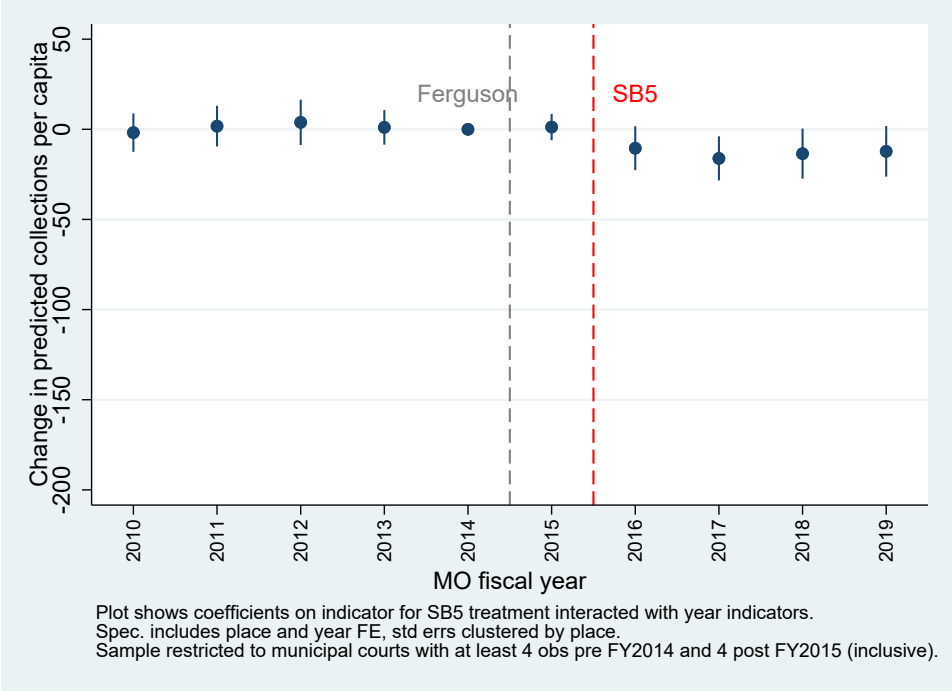


Figure 16: statewide Racial Analysis

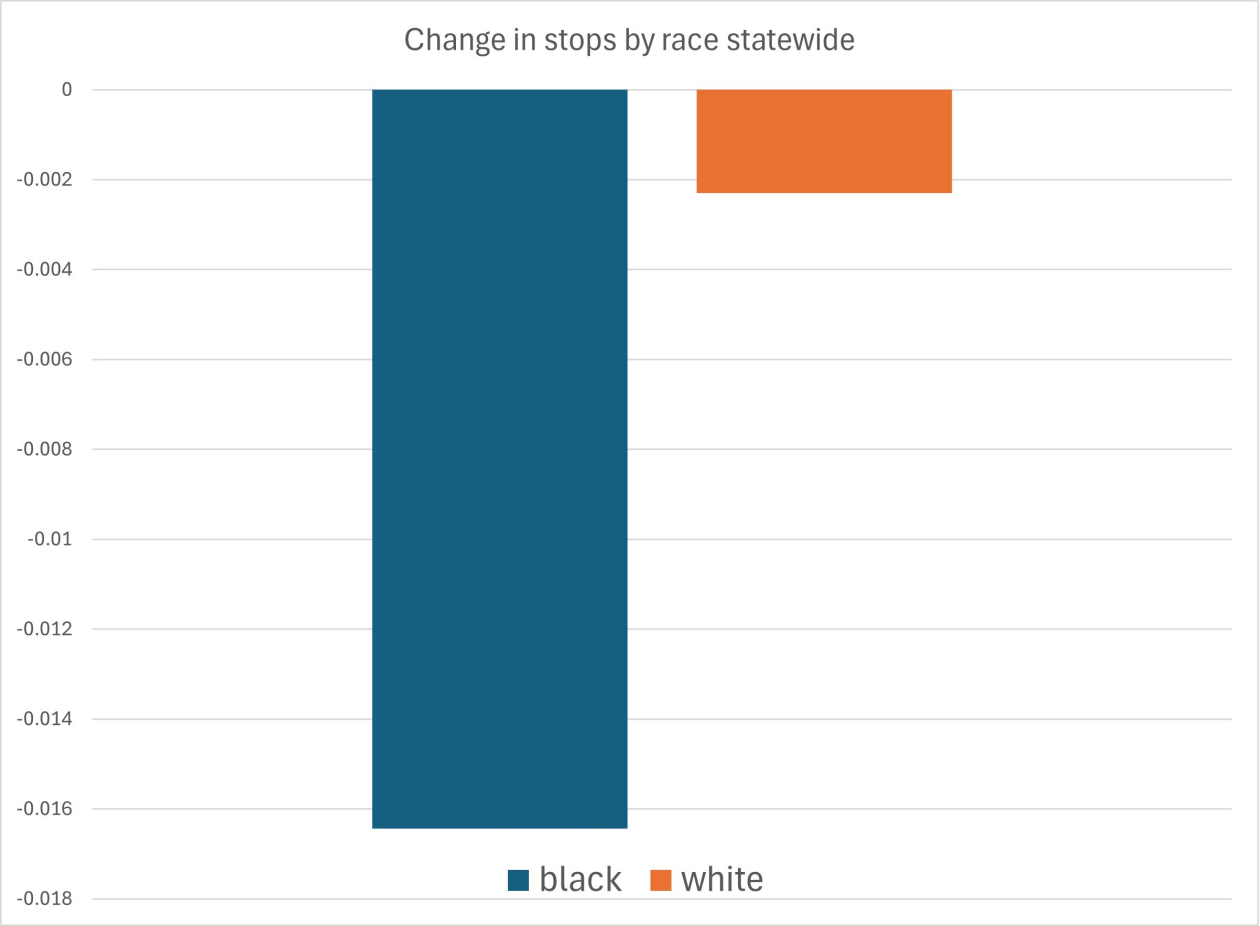


Figure 17: Racial Analysis for Treated Municipalities

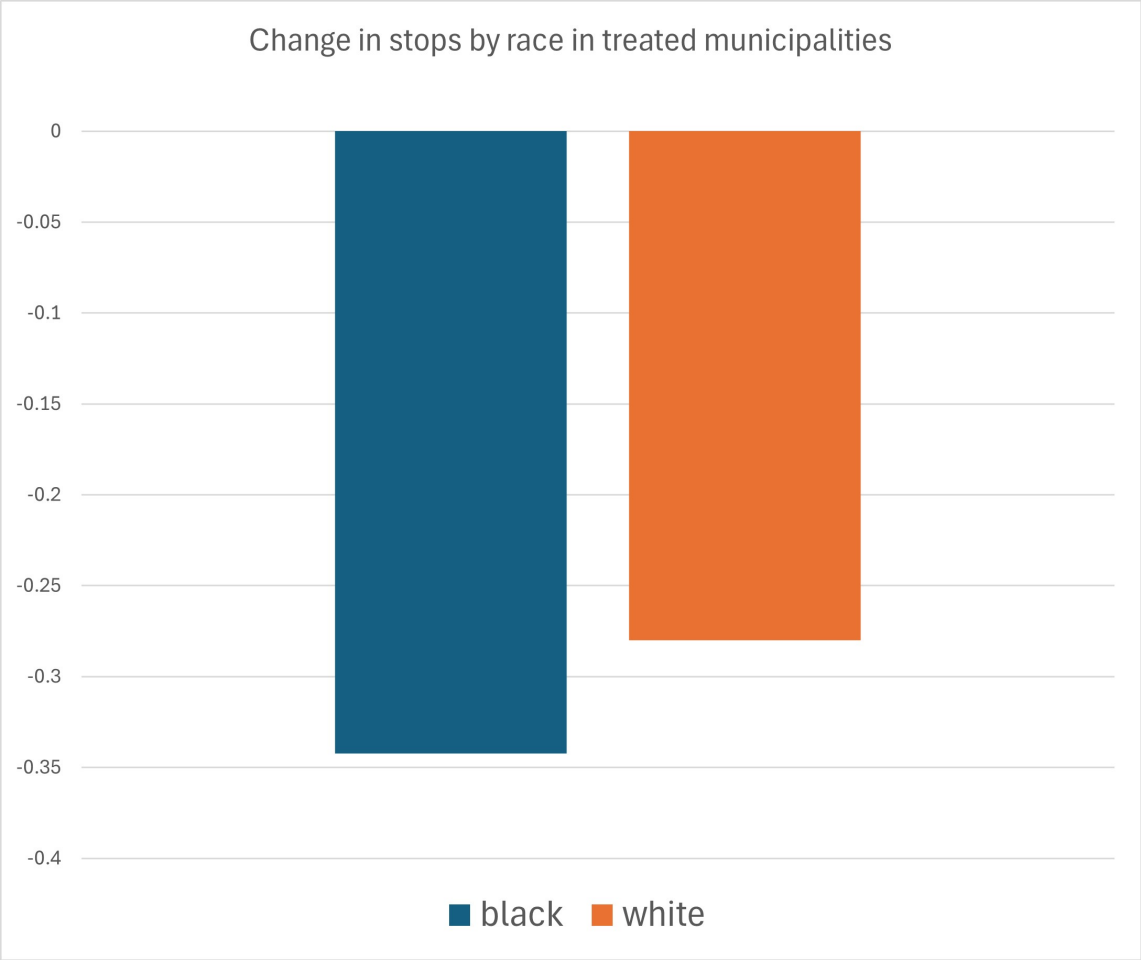


Table 1: Collections and SB5

	(1) Coll. pc	(2) Coll. pc	(3) Coll. pc	(4) Coll. pc	(5) Coll. pc
Treated X Post SB5	-143.54***			-130.33***	-143.26***
	(27.05)			(26.31)	(32.64)
Treated Cts X Post SB5		-210.39***			
		(47.36)			
Treated Pred X Post SB5			-167.94***		
			(29.42)		
Observations	3787	3787	3538	3535	3428
Clusters	385	385	357	360	348
Adjusted R2	0.84	0.85	0.85	0.84	0.86

Dependent variable in cols 1-5 is total collections per capita; col 5 in logs.
All specifications include municipality and year FE.
Standard errors clustered by municipality.
Column 1 shows baseline results.
Column 2 uses a continuous treatment; col. 3 uses a treatment predicted from financial data.
Column 4 includes county X year FE; column 5 drops cities with pop<500.

Table 2: Stops and SB5

	(1) Stops pc	(2) Stops pc	(3) Stops pc	(4) Stops pc	(5) Stops pc
Treated X Post SB5	-0.39***			-0.41***	-0.36**
	(0.12)			(0.15)	(0.15)
Treated Cts X Post SB5		-0.79***			
		(0.19)			
Treated Pred X Post SB5			-0.34**		
			(0.16)		
Observations	4056	4056	3753	3758	3661
Clusters	377	377	349	350	338
Adjusted R2	0.82	0.82	0.81	0.80	0.83

Dependent variable in cols 1-5 is stops per capita; col 6 in logs.
All specifications include municipality and year FE.
Standard errors clustered by municipality.
Column 1 shows baseline results.
Column 2 uses a continuous treatment; col. 3 uses a treatment predicted from financial data.
Column 4 includes county X year FE; column 5 drops cities with pop<500.

Table 3: Examining the 20% Cap on Municipal Revenue from Fines and Fees

	Panel A: Alternative Samples				
	(1)	(2)	(3)	(4)	(5)
Collections	-143.54***	-162.17***	-101.39***	-183.94***	-139.90***
	(27.05)	(37.15)	(37.23)	(62.64)	(30.46)
Stops	-0.39***	-0.47	-0.39***	-0.31	-0.43**
	(0.12)	(0.30)	(0.12)	(0.20)	(0.18)
Sample	All	Inside STL	Outside STL	Interstate	4th Quartile
$\bar{Y}_{collections}$	57.0	100.3	46.0	71.1	149.1
$\bar{Y}_{stops}$	0.46	0.59	0.44	0.53	1.1
Municipalities	385	77	308	129	101
	Panel B: Alternative specifications				
	(1)	(2)	(3)	(4)	(5)
Collections	-143.54***	-129.58***	-129.36***	-98.64***	-134.13***
	(27.05)	(26.6)	(27.33)	(22.24)	(25.7)
Stops	-0.39***	-0.41***	-0.47***	-0.41***	-0.41***
	(0.12)	(0.13)	(0.15)	(0.13)	(0.13)
Municipality FEs	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes
St. Louis × Time FEs	No	Yes	No	No	No
Share black × Time FEs	No	No	Yes	No	No
Warrants × Time FEs	No	No	No	Yes	No
Law-suits × Time FEs	No	No	No	No	Yes

Notes: This table presents our baseline result, as well as a number of robustness checks meant to probe the relative importance of SB5 versus increased scrutiny triggered by the shooting of Michael Brown. Rows labeled "Collections" used collections per capita as the outcome variable; rows labeled "Stops" used stops per capita. Panel A presents results for various alternative samples. Column (1) contains our baseline result. Column (2) restricts to municipalities contained inside the city of St. Louis and St. Louis County. Column (3) restricts to municipalities not contained inside the city of St. Louis or St. Louis County. Column (4) restricts to municipalities with that contain an interstate highway. Column (5) restricts to municipalities that were in the fourth quartile of fines-per-capita at baseline. Due to differences in missingness across outcome variables, the number of municipalities used in the "stops" regressions is smaller than those used for "Collections"; however, the differences are small (<5% for most cases except column 2, where there are 54 municipalities in the stops regression). Panel B presents results using alternative regression specifications. Column (1) presents our baseline results. Column (2) fully interacts the time fixed effects with an indicator for whether or not the municipality is in the City of St. Louis or St. Louis County. Column (3) fully interacts the time fixed effects with the black population share as measured in the 2010 Census. Column (4) fully interacts the time fixed effects with the number of warrants issued at baseline. Column (5) fully interacts the time fixed effects with an indicator that takes a value of 1 if the municipality was ever the subject of an Arch City Defenders law suit. Standard errors are clustered at the municipality level.

Table 4: Additional Evidence

	(1) Ln coll.	(2) Ln stops.	(3) cites pc	(4) warnings pc	(5) ln B stops	(6) ln W stops
Treated X Post SB5	-0.672***					
	(0.123)					
Treated X Post SB5		-0.180*	-0.378***	-0.047	-0.268*	-0.265***
		(0.095)	(0.118)	(0.050)	(0.142)	(0.092)
Observations	3679	4056	4056	4056	3725	4054
Clusters	383	377	377	377	368	377
Adjusted R2	0.92	0.90	0.80	0.78	0.94	0.89

Treatment is SB5.
All specifications include municipality and year FE.
Standard errors clustered by municipality.

9 Online Appendix

9.1 Figures and Tables

Figure A1: Share black stops versus share black residents

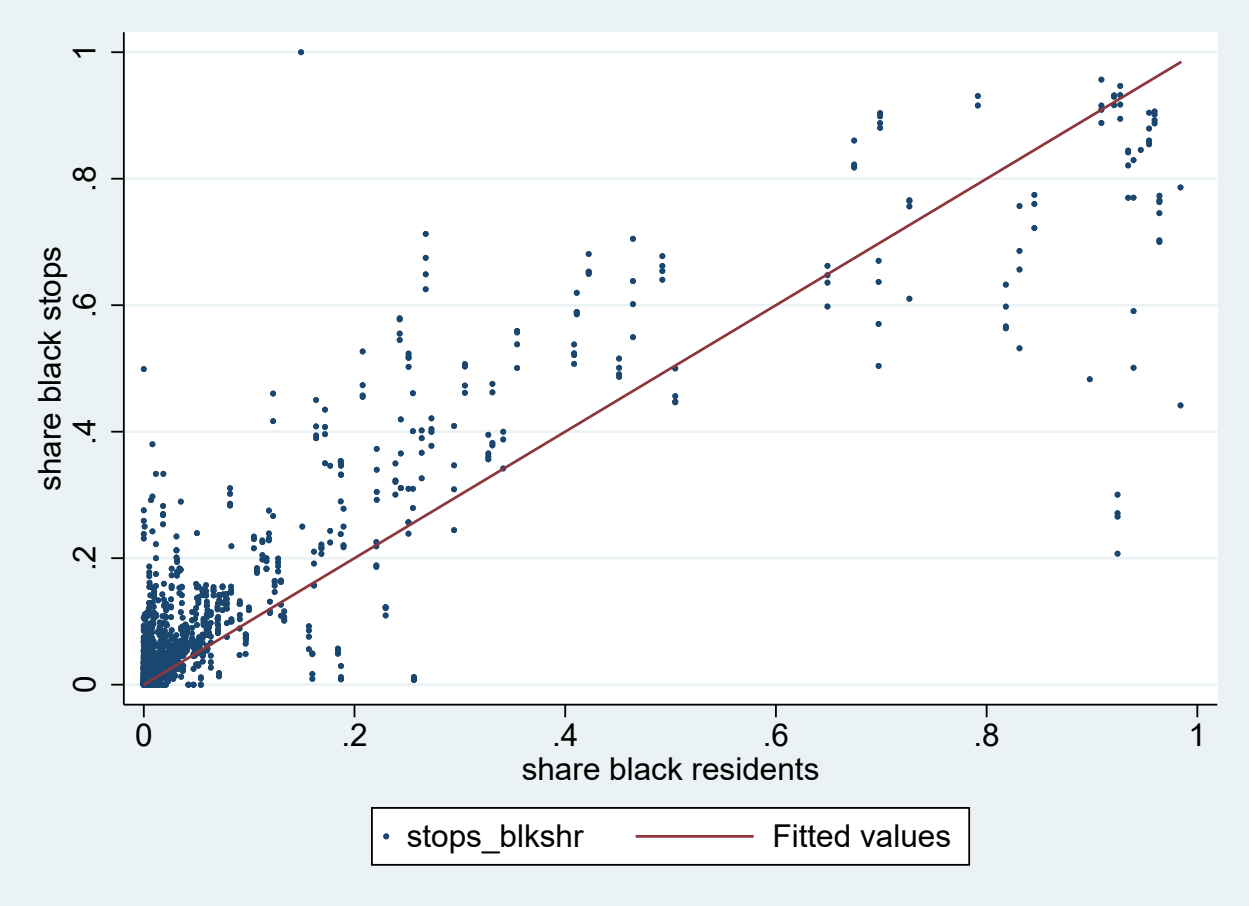


Figure A2: Share black stops versus share black residents

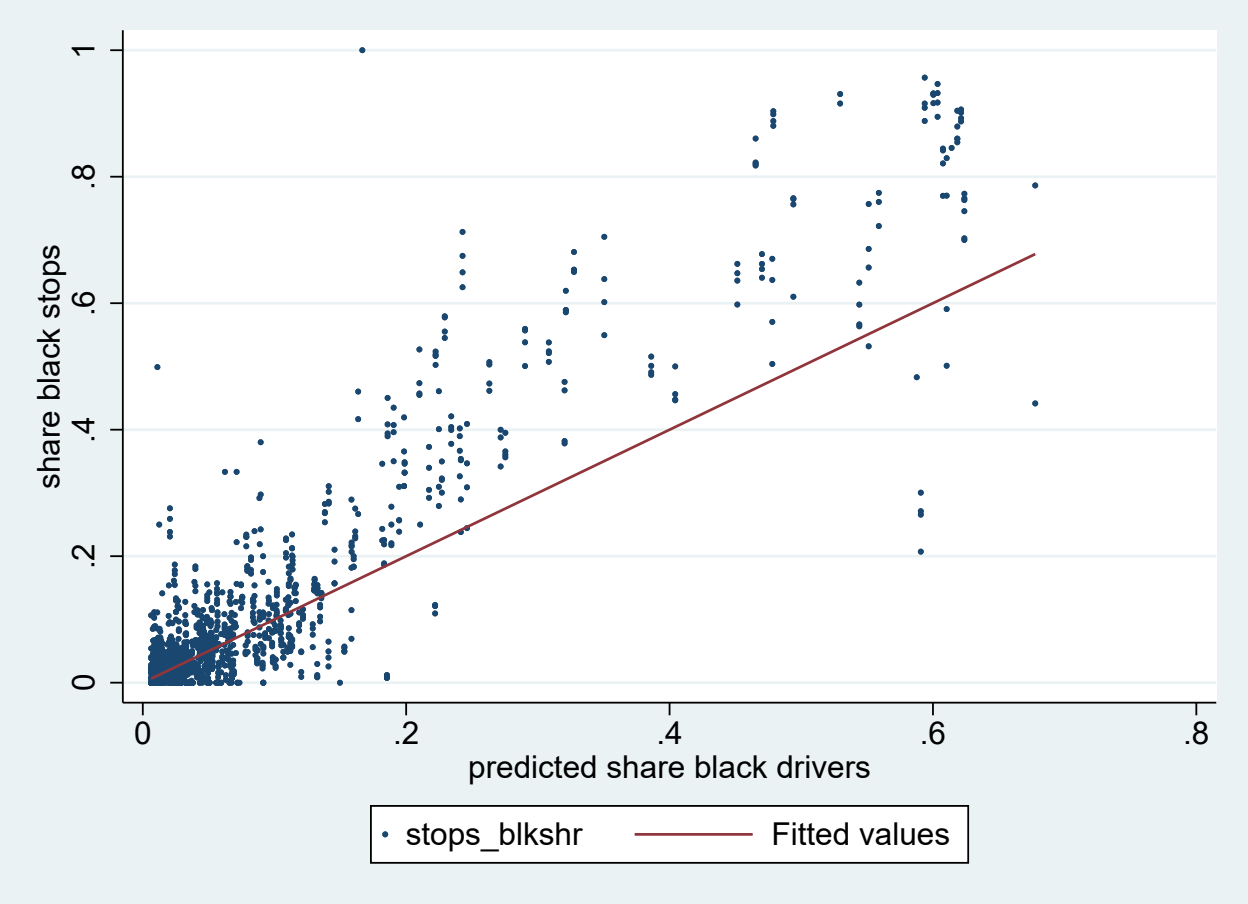


Table A1: Summary statistics

	(1)	(2)
	0	1
	mean/sd	mean/sd
Population	9142.955	1365.067
	31546.081	2104.262
Black percent	0.077	0.280
	0.182	0.384
Area square km	16.189	2.299
	47.557	2.830
intst km/area sqkm	0.147	0.295
	0.315	0.569
Warrants issued pc	0.104	1.339
	0.283	3.583
Arch city civil suit	0.015	0.111
	0.122	0.318
Observations	399	45

Table A2: Statewide Racial Implications of SB5

	white control	**white SB5**	**black control**	**black SB5**	**total white**	**total black**
annual stops (1000s)	708.52	42.20	194.52	21.22	750.72	236.72
state pop (1000s)					4958.77	693.39
baseline stops pc					0.1514	0.3414
SB5 reduction	0.00	0.27	0.00	0.27		
post SB5 stops	708.52	30.80	194.52	15.49	739.33	225.33
post SB5 stops pc					0.1491	0.3250
change in stops					-0.0023	-0.0164

Table A3: Racial composition regression analysis

	(1) black share	(2) black share	(3) black share
Muni black share	0.7506***	0.9027***	0.7292***
	(0.05)	(0.05)	(0.05)
County black share	0.5421***		
	(0.06)		
Constant	0.0087***	0.0335***	0.0161***
	(0.00)	(0.00)	(0.00)
Observations	1907	1907	1907
Adjusted R2	1	1	1

Dependent variable is black share stops.
Years cover 2010-2013
Standard errors clustered by municipality.
Column 3 includes county FE

Table A4: Racial Implications of SB5 in Treated Municipalities

	white	**black**
average annual stops (1000s)	1.1253	0.5658
adjusted local population (1000s)	1.0845	0.4464
baseline stops pc	1.0376	1.2676
after 27 percent reduction	0.7574	0.9253
difference	-0.2801	-0.3422

9.2 Policing Budgets and Workforce

In this section we explore whether the observed decline in policing activity after SB5 could be the result of decreased funding for policing. We use data from the Census of State and Local Law Enforcement Agencies (CSLLEA) for 2000, 2004, 2008, and 2018 and focus on measures of police department operating budgets and the size of the workforce (officer counts).[21]. To understand whether police resources declined systematically in treated municipalities after the passage of SB5, as well as the events in Ferguson, we first run event study specifications. We regress police resources in year t for municipal police department i on year indicators interacted

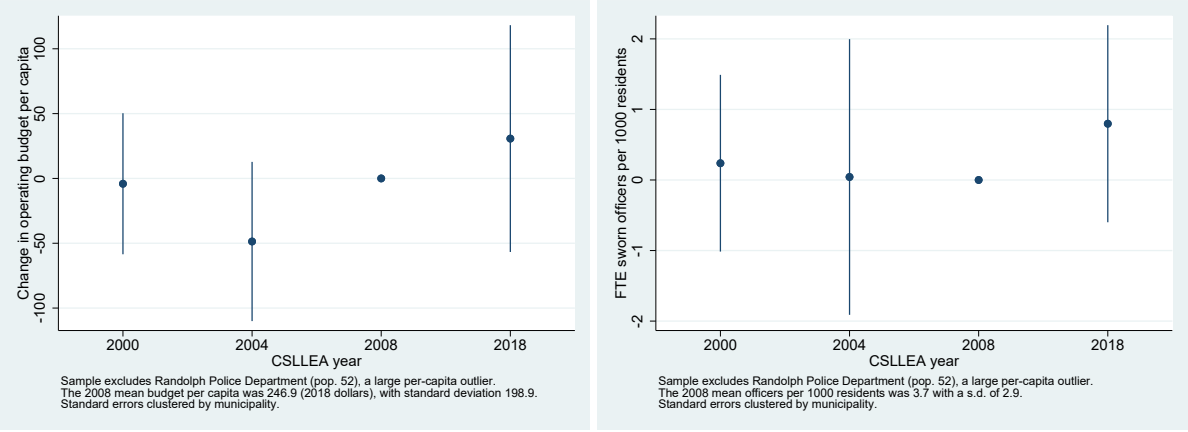
[21]The CSLLEA was conducted in 2014 but was never released due to data collection issues https://catalog.data.gov/dataset/law-enforcement-agency-roster-lear-2016-31301

with our SB5 treatment variable, D_i, and include municipality η_i and η_t year fixed effects.

$$Y_{it} = \sum_{k \in T} \beta_k D_i \times \mathbf{1}(t = k) + \eta_i + \eta_t + \varepsilon_{it} \tag{1}$$

In the above equation, T is the set of CSLLEA years (2000, 2004, 2008, 2018) and we normalize β_{2008} to zero; note that 2018 is the only year in the CSLLEA data after Ferguson and SB5. In Figure A3 we show event study plots for operating budget per capita and officers per 1000 residents, using a measure of full-time equivalent sworn officers.[22] In both plots we see no evidence of a decline in operating budgets or officer counts for treated municipalities relative to control municipalities between 2008 and 2018.

Figure A3: Police department operating budget and officer count: event studies



(a) Operating budget p.c.

(b) Officers per 1000 residents

In Table A5 we present results from the two-way fixed effect version of equation 1 by collapsing the year indicators into a single post indicator, with post defined as 2018, and estimating a single β coefficient on treatment status interacted with post. In columns 1 and 3 we show results for operating budget per capita and officers per capita, with results similar to the event study plots in Figure A3. Since per capita transformations tend to give a large weight to places with small populations, in columns 2 and 4 we show results using the natural logarithm of operating budget

[22] We focus on "sworn officers," which are officers with the power to make arrests. We calculate the count of full-time equivalent officers in a department as the sum of full time officers plus 0.5 times the number of part-time officers, following the methodology used in Bureau of Justice Statistics reports (as an example, see **?**). We also exclude observations from Randolph Village for both dependent variables. This municipality had a 2010 population of 52 people but large police resources and therefore has an outsize influence on per capita regression coefficients.

and officer count.[23] Again, the coefficients are positive and small, suggesting that policing resources did not decline differentially for treated municipalities after Ferguson and the passage of SB5.

Table A5: Police department operating budget and officer count

	(1) Budget pc	(2) Log budget	(3) Officers pc	(4) Log officers
treat_post	49.613	0.015	0.704	0.047
	(45.622)	(0.070)	(0.473)	(0.063)
Observations	1324	1325	1404	1407
Adj R-sq	0.79	0.94	0.84	0.95
Clusters	367	368	374	375

Notes: Randolph Village, population 52, is a large outlier in per capita measures and is excluded from the sample in cols 1 and 3. The dependent variable in column 1 is operating budget per capita. The dependent variable in column 2 is the natural logarithm of the operating budget and the sample includes all observations. Column 3 shows full time equivalent sworn officers per 1000 residents; column 4 is the natural logarithm of full time equivalent sworn officers and includes all observations.

9.3 Inferring Resident Stops by Race

A key ingredient into the racial implications of policing for profit involves measuring the disparity index at the municipal level: the fraction of black stops relative to the fraction of black drivers. We do not observe the fraction of black drivers. However, under a set of assumptions and using only data on the racial composition of stops and the population, one can adjust the population to better reflect the racial composition of drivers. The basic idea is that, in the absence of non-resident drivers and in the absence of police bias, the relationship should follow a 45-degree line. If drivers fully circulate across the state, by contrast, the relationship should follow a horizontal line. This allows us to infer the share of stops involving residents, allowing us to then adjust the local population to reflect the racial composition of drivers.

To formalize this idea, let s denote the share of black stops in a place and let d denote the share of black drivers. then $b = s/d$ is the bias, or disparity index. Alternatively we can say that the share of black stops is given by:

$$s = bd. \tag{2}$$

For non-resident drivers, we distinguish between drivers from the same county as the municipality and residents from the rest of the state. Let c be the fraction black in the county and let

[23] For these log specifications we also keep Randolph Village in the sample.

r be the fraction of drivers from within the county but not the municipality. Then, we have that the share of black drivers is given by a weighted combination of resident drivers, drivers from the same county, and drivers from the rest of the state.

$$d = Rm + rc + (1 - R - r)o \tag{3}$$

plugging this into $s = bd$, we have that:

$$s = b(1 - R - r)o + bRm + brc \tag{4}$$

Thus, a regression of s on m and c yields the following:

$$\beta_0 = b(1 - R - r)o \tag{5}$$

$$\beta_1 = bR \tag{6}$$

$$\beta_2 = br \tag{7}$$

As shown in column 1 of Table A3, We estimate $\beta_0 = 0.0087$, $\beta_1 = 0.7506$, and $\beta_2 = 0.5421$.[24]

Then, solving these three equations for the key model variables (b, R, and r), we have that:

$$b = \beta_1 + \beta_2 + (\beta_0 / o) \tag{8}$$

$$R = o\beta_1 / (o\beta_1 + o\beta_2 + \beta_0) \tag{9}$$

$$r = o\beta_2 / (o\beta_1 + o\beta_2 + \beta_0) \tag{10}$$

Plugging in our key coefficients yields b = 1.3718, R = 0.5472 and r = 0.3952. That is, 55 percent of drivers are residents, 40 percent are non-residents but within-county, and the remaining 5 percent are from outside of the county. In addition, our estimate of bias is 1.3718, meaning that black drivers are 37 percent more likely to be stopped than white drivers.

Using these estimates, we can adjust the share of blacks in the municipality to reflect non-residents who are stopped. More specifically, We predict the share of black drivers in the municipality using the expression for the this share (s) above. As shown in Figure A2, the disparity index, the ratio of share of black stops to the share of blacks in the adjusted population, is no longer systematically understated for places with many black residents.

[24] For comparison purposes, we drop the county black share in column 2. As shown, this increases the importance of the municipal black share. As a robustness check, we include county fixed effects in column 3. As shown, the coefficient on municipal black share is similar to that in our baseline results in column 1.