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

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Abstract

In the United States, policing can be a major source of municipal revenue, creating incentives for 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. This decline is concentrated among municipalities with the motive and opportunity to engage in policing for profit. Motivated by these findings, we focus on a reform that changed revenue incentives by requiring municipalities to remit to the state government any revenue from policing above a threshold. We find that municipalities treated by the reform significantly reduced both collections and stops; as a result, the reform can explain as much as 50 percent of the post-Ferguson declines in collections. We find a corresponding decline in public safety; however, the impact is modest. We also find that the reduction in stops is attributed to white drivers, with no change for black drivers. Given that African Americans in Missouri are stopped at higher rates than white Americans at baseline, the reform, if anything, exacerbated existing racial disparities. Taken together, our findings highlight how revenue motives in law-enforcement can distort the criminal justice system and how a simple reform that realigns incentives can create large changes in policing.

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> *"I wish to take this opportunity to remind you that the tickets that you write do add to the revenue on which the police department budget is established and will directly affect pay adjustments at budget time... Please make a self-evaluation of your work habits and motivations, then make the changes that you see that will be fair to yourself and the city."*
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> — John Gwaltney, Mayor of Edmundson, Missouri, in a memo to the Edmundson Police Department

1 Introduction

In the United States, fines and fees from the criminal justice system are often an important source of municipal revenue.[1] Critics have argued that an over-reliance on law enforcement to fund government expenditures creates incentives at odds with the core objective of policing: public safety. 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. As an extreme example, a few months prior to the shooting, the mayor of the nearby town of Edmundson wrote to the Police Department and *explicitly* linked upcoming police salary raises to revenue from tickets (Missouri Senate, 2015).

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, providing ample opportunity and motive to engage in "tax exporting" by aggressively targeting non-resident drivers. Across the United States, there are 18,000 different state and local law enforcement agencies currently operating 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," (Banks and Hendrix, 2016). Indeed, many municipalities in the United States raise a substantial share of their operating budget from law enforcement: As of 2019, more than 700 municipalities were collecting at least 10 percent of their revenue from fines and fees.[2]

Our goal is to empirically measure how revenue incentives created by the legal and regulatory environment shape police behavior, as well as their downstream impact on public safety and

[1] https://www.nytimes.com/2021/10/31/us/police-ticket-quotas-money-funding.html (accessed 10/20/2025)
[2] https://www.nytimes.com/2021/10/31/us/police-ticket-quotas-money-funding.html (accessed 10/20/2025)

racial disparities. Our setting is Missouri, where the shooting of Michael Brown and subsequent investigation led to both public outrage and a series of policy reforms intended to curb the practice.[3] We seek to address two key research questions. First, how did revenue-driven policing change after Ferguson? And 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 that limited incentives around revenue generation? 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, the universe of motor vehicle accident reports from the Missouri State Highway Patrol, 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 crossed by interstate highways experience drops that are over 50 percent larger than average. 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 fines and fees in excess of 20 percent of general revenue be remitted to the state, limiting

[3]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 fine and fees, including allowing municipalities to explicitly budget for future revenue obtained from them. For a summary of current state level laws, see Institute for Justice (2021).

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, dropping from one to zero in those places raising more than 20 percent at baseline. 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 ($105 less per resident) and stops (36 fewer stops per hundred residents) in municipalities 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 $105 (SE = 18.9) relative to a baseline collections of $246 per resident in the average treated municipality.[4] Taken at face value, our estimates imply that SB5 was responsible for as much as one-half of the overall decline in collections over the time series, an amount totaling over $41 million during the subsequent five-year period.

Why did revenues decline? 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. This would be the case if, for example, the places raising more than 20 percent of their revenue at baseline were differentially subject to public outrage over the shooting. For policy implications, it is important to distinguish between these two potential mechanisms.

We argue that the cap on retained revenue is the primary mechanism. To support this claim, we offer four pieces of evidence. First, we note that the results are robust to conducting the analysis within groups that had more or less exposure to the outrage. For example, we can flexibly control for racial composition, which we expect to be a strong predictor of scrutiny, or for variables like interstate highway presence that increase the incentives for policing for profit and might therefore lead to increased public focus; we can also account for other, more direct, proxies for outrage, such as law-suits filed against police departments in the aftermath of the killing; and we can even restrict the sample to municipalities outside of St. Louis County, which are 93 percent white and far from the center of controversy. In all cases, the results are similar. Second, we note that the dynamics of the treatment effects are consistent with SB5. While there is a small drop in collections in the year after Ferguson, the largest drop occurs in 2016, the precise year the cap was implemented, and is sustained thereafter. Third, we note that the magnitudes suggest that revenue-per-stop was the key margin of adjustment. If scrutiny led to a differential reduction in policing effort in treated municipalites, we would expect to find a proportional decline in stops and revenues; instead we find a 67 percent decline in revenue which greatly exceeds the corresponding 18 percent decline in stops. This is consistent with a reduced incentive to collect

[4]The average municipality in our data collected $68.8 per resident at baseline.

revenue conditional on a stop. Finally, we note that the entirety of the 18 percent decline in stops is driven by those stops that yield revenue via a citation; there is little to no impact on stops that results in warnings, which do not generate revenue. Taken together, we argue that these patterns are more consistent with the limit on retained revenue and corresponding change in incentives as the primary mechanism.

The finding that the cap on retained revenue reduced policing for profit raises secondary questions about the downstream impact of the law on other outcomes. For example, it could be that the decline in ticketing reduced the expected cost to drivers of engaging in dangerous behavior (e.g. drunk driving or speeding), which could lead to declines in public safety. In addition, given the focus of the DOJ report on the racial equity implications of policing for profit, it is natural to ask whether the decline in collections had different implications for black and white residents. Thus, we conclude our analysis by exploring these secondary questions.

We also find evidence that the cap on retained revenue had a modest impact on public safety. To explore this issue, we use publicly available data from the Missouri State Highway patrol covering the universe of motor vehicle accidents over this period. A key advantage of this data is that it is recorded at the incident level, which allows us to explore changes in trends at finer temporal resolutions (i.e. monthly, rather than yearly). Based upon event studies around the implementation of SB5, we find a modest increase (0.515) in the average number of vehicles per month that are involved in accidents. However, we caution that the standard errors are large enough that we cannot rule out a moderately sized increase. We find similar results for other measures of traffic safety (e.g. frequency of injuries or their severity), albeit with varying degrees of precision. Overall, these findings are consistent with the idea that reducing revenue incentives for law-enforcement also led to a small deterioration in public safety.

We then turn to the implications for racial equity. Given that black Americans were policed at higher rates than white Americans prior to Ferguson, proponents of Sb5 pointed to the potential for reducing racial disparities in policing by race. To examine these issues, we use the fact that our stops data includes information disaggregated by the race of the driver. More specifically, we use the data on stops to decompose the overall reduction in stops per capita into stops of white drivers per capita and stops of black drivers per capita. Here we find that, inconsistent with proponents of Sb5 arguing that the reform might reduce racial disparities, the reduction in stops can be attributed entirely to a reduction in stops of white drivers, with no change in stops of black drivers. Given that black drivers were policed at higher rates at baseline, as noted above, Sb5 thus seems to have, if anything, exacerbated these preexisting disparities between black and white drivers in Missouri.

2 Related Literature

Our paper contributes to a growing, interdisciplinary literature on policing for profit by being the first to study how relevant policy reform can shape police behavior. Prior work studies revenue-driven policing via incentives created by general fiscal conditions and preexisting laws (e.g. Carpenter et al., 2021; Garrett and Wagner, 2009; Su, 2020, 2021; Subramanian et al., 2022). Of particular note is Makowsky and Stratmann (2009), who show that police in Massachusetts are more likely to issue tickets to out-of-town drivers when fiscal conditions are tight, and Makowsky et al. (2019) who document how certain types of arrests are differentially higher when there are local deficits in states that allow for retention of forfeitures. Also closely related is Harris et al. (2020), who explore the racial implications of fiscal conditions and traffic enforcement in Missouri prior to the shooting of Michael Brown. Much of this work also considers downstream implications for public safety (e.g. Makowsky and Stratmann, 2011). There is also closely related research on civil asset forfeitures which finds that both local governments and police agencies react to new revenue streams in a manner that is consistent with their financial interests (e.g. Baicker and Jacobson, 2007; Holcomb et al., 2011).[5] Our central contribution is to examine changes in policing following the introduction of new laws and other policy reforms specifically designed to limit policing for profit. As as result, our identification strategy—which leverages policy variation in a differences-in-differences research design—is also distinct from prior work, which generally exploits cross-state panel variation in fiscal conditions.

More broadly, our study contributes to a large literature on policing in the United States by directly connecting incentives from the regulatory environment to police behavior. Traditionally, much of the economics literature on policing in the US has focused on the impact of force size or police presence on crime (e.g. Chalfin et al., 2022; Chalfin and McCrary, 2018; Chen et al., 2025b; Di Tella and Schargrodsky, 2004; Evans and Owens, 2007; Klick and Tabarrok, 2005; Mello, 2019; Owens and Ba, 2021; Weisburst, 2019). More directly related to the present study are a series of papers that examine police misconduct, aggressive enforcement, and/or the impact of any subsequent investigations or attempts at accountability (Ang, 2021; Ang et al., 2025; Ang and Tebes, 2024; Gonçalves et al., 2025; Jácome, 2022; Mikdash and Zaiour, 2024). Of particular note is Devi and Fryer Jr (2020), who find that "Pattern-or-practice" investigations that are preceded by viral incidents of police force lead to a decline in police activity and an increase in homicides.[6] While that finding on police activity is echoed in other research, the results on homicide stand in contrast to a number of studies that find little evidence of a "Ferguson" effect

[5] An exception is Billy (2020), who finds that local governments did not react to a temporary pause in federal civil asset forfeitures.

[6] Shi (2009) finds similar results after a high-profile police shooting that occurred in Cincinnati in 2001.

on crime or traffic fatalities in major US cities (Campbell, 2025; Chalfin and Massenkoff, 2022; Pyrooz et al., 2016; Shjarback et al., 2017). Finally, there is also a recent body of work that takes a behavioral view of the underlying determinants of police behavior (Adger et al., 2024; Annan-Phan and Ba, 2024; Chalfin and Gonçalves, 2024; Dube et al., 2025; Holz et al., 2023; Ba et al., 2021b; Mello et al., 2025). Most closely related is Rivera and Ba (2025), who find that police oversight can cause a decline in reported misconduct. Our paper adds to the literature on policing in the United States by highlighting the broader legal and regulatory environment as a key margin that shapes the current equilibrium.

Our study also contributes to the literature on racial disparities in the criminal justice system by highlighting how reforms around policing for profit might have unintended consequences around policing by race. Racial disparities emerge at every phase of the criminal justice system—from police contact to charging, pretrial detention, plea bargaining, sentencing, and ultimately incarceration (National Academies of Sciences and Medicine, 2023). Sociological and criminological views tend to emphasize a broad array of social factors, which can accumulate and interact in complex ways (e.g. Baumer, 2013; Phelps and Pager, 2016). Recent research in economics tends to leverage quasi-experimental methods and study more specific margins, including policing (e.g. Anwar and Fang, 2006; Antonovics and Knight, 2009; Ba et al., 2021a; Coviello and Persico, 2015; Chen et al., 2025a; Fryer, 2019; Hoekstra and Sloan, 2022; Knowles et al., 2001; Lieberman, 2024), bail decisions (e.g. Arnold et al., 2018, 2022; Kleinberg et al., 2018), and sentencing / incarceration (e.g. Abrams et al., 2012; Rehavi and Starr, 2014).[7] Of particular note is Goncalves and Mello (2021), who find that 42% of officers in the Florida highway patrol engage in racial discrimination when levying fines via traffic tickets — an outcome with important implications for downstream economic well-being (Finlay et al., 2024; Mello, 2025). Also closely related is Luh et al. (2025), who find evidence that a sharp increase in parking fines for failure to purchase a vehicle registration in Chicago led to differential enforcement in black neighborhoods and ultimately increased racial gaps in financial instability. We contribute to this literature by highlighting how reforms that were both designed to curb policing for profit and motivated by racial disparities might have unintended racial consequences, and actually *increase* these preexisting disparities.

Given our analysis of small municipalities and non-resident drivers, we contribute to a literature on the advantages and disadvantages of decentralization in the provision of public services. In particular, given that non-residents are unable to hold local officials accountable at the ballot box, this can lead to inefficient policies, with an over-reliance on revenue sources paid by non-

[7]The economic literature on racial disparities is too large to fully cover here. For additional review, see Lang and Spitzer (2020).

residents. In the context of higher education, for example, Knight and Schiff (2019) document that public universities charge much higher out-of-state tuition, leading to distortions in student choices. We contribute to this literature via an examination of a state-level policy that capped retained revenue from policing, reducing incentives around revenue generation, and the associated responses by local police departments.

3 Background: Ferguson, the DOJ Report, and SB5

On August 9, 2014, an 18 year-old, unarmed, Black man named Michael Brown was killed by police officer Darren Wilson in Ferguson, Missouri. The confrontation that led to Brown's death erupted after Brown stole cigarettes from a convenience store. Outrage over the shooting led to demonstrations, which began on the following day. Some protesters directly confronted police and ultimately damaged storefronts near the location of Brown's death. Additional protests continued throughout the month of August, restarted for a short period in November 2014 after a grand jury declined to indict officer Wilson, and occurred once again in August 2015 on the anniversary of Brown's killing. The incident, and subsequent protests, attracted international attention and press coverage.[8]

The U.S. Department of Justice investigated both the details of the shooting and the practices of the Ferguson police department. This investigation led to two reports, both issued in March of 2015. Importantly, the DOJ report on practices specifically highlighted revenue incentives and policing for profit as contributing factors, concluding that "Ferguson's law enforcement practices are shaped by the City's focus on revenue rather than by public safety needs," and, as a result, "disproportionately harm Ferguson's African-American residents," (Department of Justice, Civil Rights Division, 2015). The subsequent attention to revenue driven policing led to calls for reform both nationally and locally (The Washington Post Editorial Board, 2015; The New York Times Editorial Board, 2015).

In an effort to respond to the outrage and curb policing for profit, the state of Missouri subsequently passed Senate Bill 5 (SB5).[9] This reform took effect on January 1, 2016—or, for municipalities with non-calendar fiscal years, at the start of their next fiscal year—and limited local governments from raising more than 20 percent of municipal general revenue from traffic fines and fees. Importantly, the law *did not prevent* municipalities from collecting revenue be-

[8]For a more detailed discussion of the timeline, see relevant news coverage in Associated Press (2019), Swaine (2014), and CBS News (2015).

[9]Beyond SB5, we also note that Rule 37.04 went into effect on July 1, 2017, which further limited revenue incentives by removing the ability of Missouri courts to issue warrants and hold individuals in jail for minor traffic violations (Supreme Court of Missouri, 2016).

yond the 20 percent cap; instead, it simply required revenue in excess of 20 percent to be remitted directly to the state government (rather than being retained by the local municipality).[10] Thus, the law led to sharp changes in incentives for revenue generation, with the local share from the marginal ticket dropping from 100 percent to 0 percent in "treated" municipalities (i.e. those raising more than 20 percent of general revenue from policing at baseline), a fact we will exploit for identification in section 6. However, before turning to the research design, we first discuss the data.

4 Data and Summary Statistics

Data construction. The key variables for this study are drawn from diverse sources including the decennial census, municipal court records, municipal financial reports from the State of Missouri Auditor's Office, and vehicle stops reports from the Office of the Missouri Attorney General. Data construction begins with the universe of Census place codes in Missouri (N = 1099) as of the 2010 decennial Census.[11] From the Census data, we retain basic demographic variables including counts of total population and population by race.

Next, we incorporate publicly available information on court collections. Specifically, we wrote code to parse data on collections (fines, fees, etc.) and warrants from PDFs contained on the Missouri courts website.[12] In order to merge these records to the census file, we matched the municipal court names to Missouri census place names using a combination of string matching and manual linking. After merging, we restricted our data to the 470 municipalities that had an active municipal court (i.e. those that collected at least one fine) over our study period (MO fiscal years 2010-2019). We also excluded Ferguson.[13] Court statistics contained in this data include information on municipal court collections, cases, and warrants, by municipality and state fiscal year from 2010-2019. Notably, the state fiscal year runs July 1-June 30, a distinction that will be important in subsequent analyses for aligning event times appropriately whenever collections are the outcome.[14] Importantly, the Missouri court statistics record all the revenue collected from court fines and fees, which includes revenue that would be retained by the municipality and

[10] The original SB5 that passed on July 5, 2015 further limited municipalities in St. Louis County to 12.5 percent, rather than the 20 percent applied to all other municipalities. This was struck down by the Missouri Supreme Court in 2017, making the limit 20 percent for all municipalities in the state. See discussion in Rubinstein (2021).

[11] Specifically, we obtain the extract for our study from the IPUMS National Historical GIS project: www.nhgis.org/documentation/tabular-data.

[12] The raw records are contained here: www.courts.mo.gov

[13] This choice is out of an abundance of caution since Ferguson was subject to a federal consent decree after the DOJ investigation.

[14] For example, the Ferguson shooting occurred during fiscal year 2015, which began on July 1, 2014, a few months before the shooting.

revenue that would have to be remitted to the state in the event that the municipality was over the SB5 20% limit. Thus, this data captures total collections associated with policing.

SB5 placed a cap on the share of general revenue from fines and fees. We use the court statistics to measure the numerator of this ratio and measure general revenue, the denominator, using comprehensive annual financial reports (CAFRs) for each municipality from the MO auditor's office.[15] We hired a data entry firm to manually input the statement of revenues, expenditures, and changes in fund balances for each municipality's CAFR in fiscal years 2011-2013. We measure general revenue in a given fiscal year as the sum of revenue from a municipality's "General Fund."

We then add publicly available data on traffic stops from the Office of the Missouri Attorney General. These data come from annual "Vehicle Stops Reports" and contain annual counts (separately by race) of the number of stops and stop outcomes (e.g. warning versus citation) for every law enforcement agency in Missouri.[16] We wrote additional code to transcribe the data from the "Vehicle Stops Reports" PDFs. In order to merge these data to our analysis file, we matched the law enforcement agency names to Missouri census place code names using a combination of string matching and manual linking. These data are reported for each calendar year and hence align with the various events we study using calendar time.

Finally, we incorporate a number of other supplementary data sources that allow us to measure additional, secondary concepts. Specifically, the Census of Governments[17] provides data on municipal finances; geospatial information on municipality boundaries and the network of roadways are derived from Census GIS products; and we also obtain data on traffic volume from the US Department of Transportation. For more details on the cleaning and integration of these supplementary data sources, see appendix B. In secondary analysis, we also seek to analyze the impact of SB5 on safety. To do so, we also make use of incident level data on car crashes that are publicly available from the Missouri Department of Transportation. The fact that these data are at the incident level will allow us to explore patterns at a finer level of temporal resolution than the rest of our data sources, which are reported annually, and hence the crash data require a slightly different data cleaning and merging procedure. For that reason, we defer a detailed discussion of this secondary data source until section 8.

Summary statistics. The data set we use for our primary analyses contains 470 municipalities observed over the calendar years 2009 to 2019. Summary statistics for this sample, restricted to a 2012 cross-section, are provided in table 1. The first column, labeled "All," contains the

[15] Each municipality's financial report can be downloaded from here: https://auditor.mo.gov

[16] The raw records are contained on the Missouri Attorney General's website: www.ago.mo.gov

[17] We use "The Government Finance Database" from Willamette University, which standardizes and aggregates the Census of Governments data into a more accessible format: https://my.willamette.edu/site/mba/public-datasets.

universe of municipalities in Missouri that ever had an active municipal court over fiscal years 2010-2019.[18] We use this sample to explore statewide questions related to declining municipal revenue that occurred in the aftermath of Ferguson. We measure the share of revenue in excess of the SB5 20% threshold by calculating the ratio of total collections to general revenue for each fiscal year 2011-2013, and then take the average across these three years. This averaging provides a baseline ratio that is less sensitive to yearly fluctuations in general revenue and fines and fees. We are missing all 2011-2013 CAFRs for a small number of municipalities. For these municipalities we used the general revenue measure from the 2012 Census of Governments.[19]

The "Analysis" column contains the subset of municipalities we use to explore questions related to SB5, with the columns labeled "Control" and "Treat" containing municipalities that were below (or above) the SB5 20% threshold on average over fiscal years 2011-2013. This sample differs from the sample contained in the "All" column because we restrict to municipalities that are approximately balanced, year-over-year, in terms of the existence of fines in the court records.[20] We impose this restriction to avoid econometric issues related to imbalanced panels that can be particularly salient in the context of event studies like the one we will deploy to analyze the impact of SB5.

Table 1 reveals that municipalities that raised over 20% of general revenue from fines and fees at baseline are different on a number of observable characteristics. Demographically, we find that SB5 municipalities are smaller (average population of 1,715, versus 10,826 in the control group) and disproportionately Black (25.6% versus 8.4%). This likely stems, in part, from the fact that SB5 municipalities are more likely to be located in the St. Louis area (41% versus 16%).

We also find that municipalities impacted by SB5 engaged in more traffic stops. For example, police in the treated municipalities stopped 1.617 motorists for every resident, as compared to only 0.342 in control municipalities. This is consistent with the fact that municipalities in our treatment group had a larger interstate highway presence (0.282 per square mile in the treated municipalities, relative to 0.160 in control). It is also consistent with the anecdotal evidence (described in section 3) which suggests that interstate highways facilitate opportunities to engage in policing for profit via tax-exporting, a point we return to in the next section.

Finally, we note here that table 1 reveals a large disparity in revenues collected. Municipalities that were above the SB5 threshold in 2012 collected an average of $246 per person from fines and fees, as compared to only $38 per person in the control group – a nearly 7 fold increase.

[18] As discussed in the preceding section, the one exception is Ferguson, which we exclude throughout out of an abundance of caution.

[19] For cases where we observe one or two CAFRs over this period, but not all three fiscal years, we take the average collections to general revenue ratio for the years we observe.

[20] Specifically, we retain those municipalities where we can observe fines in every year for the 4 years pre-post SB5

This is consistent with a profit motive; yet, it could also be driven by the baseline differences in demographic characteristics documented earlier. For example, it could be that reckless driving patterns in the St. Louis area necessitate aggressive enforcement in order to deter dangerous driving behavior. Thus, the baseline differences we document in this section motivate a more careful study of revenue in the aftermath of Ferguson in section 5, as well as the more formal SB5 event study research design we will explore in section 6.

5 Revenue from Fines and Fees in the Aftermath of 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 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 1a 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 1b, 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).

Taken together, the timing of the declines in collections and stops suggests that Missouri's policy reforms (e.g. SB5) may have affected revenue beyond any decline resulting from post-Ferguson scrutiny. 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 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 2a, 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 were 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 2b. 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 of revenue 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 on interstates have a particular incentive to engage in policing for profit, given that they have 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 A.1a, 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 A.1b. As is clear from the figure, even within the St. Louis metro area, there is visual evidence of higher collections in areas with interstates passing through their municipal boundaries.

Next we 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-by-St. Louis metro area). As shown in Figure 3a, 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 3b, 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—a feature which provides 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.[21] 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 A.2a, we document a pattern similar to that in Figure 3a, 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 A.2b, 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

[21] 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.

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, consistent with the idea that non-resident drivers provide 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 analysis thus far does not identify specific mechanisms. We now turn to assessing the effect of cap on retained revenue imposed by 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 an event-study framework.

7 Research Design

To identify the impact of SB5, we estimate models of the following form:

$$Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times 1_{m \in M^*} + \varepsilon_{mt} \tag{1}$$

where Y_{mt} is an outcome variable (typically court collections per capita or stops per resident) measured in municipality m in calendar (or fiscal) year t; α_m is a municipality fixed effect; $s = s(m)$ denotes whether municipality m was in the St. Louis metro area (defined to be St. Louis City plus municipalities in St. Louis County); $\delta_{ts(m)}$ is a calendar (fiscal) year by St. Louis area fixed effect; $D_\tau = 1(t - t^* = \tau)$ is an indicator that takes a value of one τ years after the killing of Michael Brown (where t^* = calendar year 2013 for stops, and t^* = fiscal year 2014 for collections); M^* denotes the set of municipalities that were above the SB5 20% cap on retained revenue at baseline; and $1_{m \in M^*}$ is an indicator variable that takes a value of 1 for all municipalities

that were above the SB5 20% cap on retained revenue at baseline. For inference, we cluster standard errors at the municipality level.

For causal identification, we rely on a parallel trend assumption. Specifically, model (1) will yield dynamic causal estimates of the impact of SB5 if the trend in municipalities where we expect the SB5 threshold was not binding (i.e. $m \notin M^*$) is an accurate counterfactual for the trend the treated municipalities (i.e. $m \in M^*$) would have experienced if the bill never passed. The key threat to identification is a correlated shock emerging from the events of Ferguson: if municipalities above the 20% threshold were differential subject to outrage over the murder of Michael Brown, this could lead us to conflate the impact of Ferguson with the impact of SB5. We return to this point in section 7.2.

Importantly, we note here that SB5 limited local revenue from policing to 20 percent of general revenue, with any revenue collected above this threshold remitted to the state. It *did not* mechanically prevent a municipality from collecting additional fines once the 20% threshold was reaching. In other words, the law implies that when a municipality crosses the revenue threshold, the local share from a marginal dollar of policing falls from 100 percent to zero. Thus, above this threshold, a municipality with pure revenue motives should respond by reducing policing, while a municipality with pure safety motives should 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.

7.1 Event-study Estimates

Figure 4 contains the key results from the event study model. We begin with panel 4a, which focuses on collections. We observe that, prior to the events of Ferguson, treated and control municipalities were evolving similarly. This is consistent with the parallel trends assumption that is necessary for causal identification. After the events of Ferguson, we observe a small decline in places treated by SB5 in fiscal year 2015—occurring before the implementation of the law—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 4a 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 5 the black line ("Raw data") shows the change in collections per capita observed in the data, similar to Figure 1a, 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 4a 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.

Turning to our data on stops, we estimate corresponding event studies. As shown in Figure 4b, there is a small (imprecise) decline in calendar year 2014, the year of the shooting, in places treated by SB5, relative to control municipalities. In 2015 there is a large decline of roughly 0.4 stops per capita that remains constant in the subsequent years 2016-2019. While the post-Sb5 decline is consistent with a reduction in policing due to the change in financial incentives, the drop in 2015 is also consistent with a reduction in policing due to the shooting in Ferguson. Given this, we cannot definitively separate the role of Sb5 from the role of Ferguson in explaining the reduction in stops in the context of this event study alone. We return to this issue of interpretation in the next section.

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 2 we show the coefficients on the interaction between a SB5 treated indicator and a Post indicator, where SB5 treatment is defined as above the 20% threshold at baseline and the Post indicator is defined as fiscal year 2016 or later for collections, and calendar year 2016 or later for stops. As shown in column 1 of Panel A, we find that collections per capita decline by $105.4 (row 1) and stops per capita decline by 0.36 (row 2) in treated municipalities, relative to untreated municipalities, after the implementation of the SB5 reform. Taken together, the reduction in collections is consistent with our evidence from the event studies, which document economically and statistically significant reductions in collections in treated places following the implementation of SB5. This evidence is again consistent with revenue-driven municipalities reducing their collections 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.

7.2 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. How-

ever, 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.[22] As shown in column 2 of Table 2 (Panel A), the estimated effects in St. Louis are larger than our main estimates in column 1 for collections, but smaller than for stops, which is imprecisely estimated within St. Louis. In column 3 we show estimates for 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. However, for these municipalities we find large and statistically significant effects close to our baseline estimates in column 1. This is consistent with the differential decline being primarily driven by SB5. 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 2, municipalities both treated by SB5 and with interstates experience a decrease in per capita collections of $135, compared to municipalities that are both untreated and have interstates. Moreover, the effect on stops per capita is nearly identical 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 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

[22] 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.

post-Ferguson scrutiny. These results are presented in Panel B of Table 2. In the second column, we 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 2, 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.[23] As shown in column 3, 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 4 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 4, including this control has little effect on our key coefficients.

As our second piece of evidence, 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 4 above, collections fell only slightly 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 5, 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, we would have expected larger relative declines in collections in 2015, the year following the Ferguson shooting.

As our third piece of evidence, 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 A.5, 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),

[23]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.

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.[24] 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 A.3, 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.

As our fourth and final pice of evidence, we examine the distinction between stops resulting in citations, which generate revenue, and stops that result 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 A.5, 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 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.

7.3 Policing Resources

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

[24] 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.

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 B.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.

8 Downstream Outcomes: Safety and Racial Equity

Our results to this point raise two, important, secondary questions. The first concerns safety. In theory, policing for profit could be justified if the incentives created by revenue sharing of fines and fines leads to increased effort on the part of law-enforcement and an overall improvement in public safety.[25] Therefore, it is important to ask whether SB5 caused downstream changes in the likelihood of safety outcomes related to traffic tickets (e.g. accidents).

The second concerns racial equity. Much of the motivation for SB5 concerned the racial implications of policing for profit. If policing for profit disproportionately impacts Black Americans, perhaps because they are more likely to live in places engaging in policing for profit or due to other explicit racial biases embedded in the criminal justice system, then policing for profit could, in part, be responsible for inequities across race groups in the frequency and nature of their contact with the criminal justice system. Therefore, it is important to ask whether the cap on retained revenue, by curbing policing for profit, improved racial equity in Missouri. In this section, we explore both of these important questions in turn.

8.1 Policing for Profit and Safety

To explore safety, we leverage incident level data covering the universe of traffic accidents in Missouri. Importantly, because this is incident level data, it will allow us to explore the implications

[25] Whether this is optimal will, in general, depend on the distortions created by the taxation that would otherwise be necessary to incentivize a level of effort by law-enforcement that balances the marginal cost and benefit. While a full treatment is beyond the scope of this paper, we note here that, if safety is the primary objective, it clearly cannot be optimal to incentivize police effort that does not lead to improvements in safety.

of SB5 at finer levels of temporal resolution than our baseline results. To take advantage of this feature, we aggregate the incident level data in two steps by: (1) spatially merging the latitudes and longitudes corresponding to each accident with municipality shapefiles from the Census; and (2) summarizing the data at the municipality by month level. Key variables we construct, all measured in per-capita terms, include the number of accidents, the number of vehicles involved in accidents, and the number of injuries. Given that the presence of highways also increases the incentive to police for profit, we also construct alternative versions of these variables that specifically aggregate the data only using incidents that occurred on a highway. See appendix B.1 for additional details regarding the cleaning, with appendix table A.3 giving summary statistics.

We begin with a simple visualization of raw trends. Panel A of figure 6 plots the average number of monthly traffic accidents per-capita for both treated, and control, municipalities. In this panel, we restrict to municipalities in the St. Louis area to facilitate visualization in light of the monthly data: In practice, SB5 required the limit on retained revenue to go into effect at the start of the fiscal year during calendar year 2016, and most municipalities in the St. Louis area share a June 30 fiscal-year end date, whereas fiscal year end dates are more heterogeneous at the statewide level. The first vertical lines denotes the date of Michael Brown's killing. The second vertical line denotes June 30, 2016, just before SB5 went into effect for treated municipalities. Best fit lines are estimated separately within segments.

Overall, there is little visual evidence of a reduction in safety in treated places following the implementation of SB5. As shown by the lines of best fit, the average number of crashes per-capita is similar between treatment and control municipalities during the three relevant time periods: before the shooting in Ferguson, after the shooting but before the start of SB5, and then following the implementation of SB5. There is some evidence of a discrete increase in accidents following the shooting, but the patterns are noisy at this monthly level of frequency and difficult to detect in the raw trends.

To explore this formally, we estimate models of the form:

$$Y_{mt} = \alpha_m + \delta_{ts(m)} + \beta D_{mt} \times 1_{m \in M^*} + \varepsilon_{mt}, \tag{2}$$

where Y_{mt} is an outcome variable (e.g. traffic accidents), measured per 1,000 residents on date t (month-by-year) in municipality m; α_m is a municipality fixed effect; $s = s(m)$ denotes whether municipality m was in the St. Louis metro area (defined to be St. Louis City plus municipalities in St. Louis County); $\delta_{ts(m)}$ is a date by St. Louis county fixed effect; $D_{mt} = 1(t - t_m^* > 0)$ is an indicator that takes a value of one for every month after the last fiscal year (t_m^*) before SB5 went

into effect for municipality m; and $1_{m \in M^*} = 1(m \in M^*)$ is an indicator that takes a value of 1 if the municipality was expected to be treated by the law ($m \in M^*$) as a result of being over the 20% threshold at baseline.

The key parameter from this regression model is β, which measures the change in trend around the date that SB5 went into effect in places where it was expected to bind relative to places where it was not expected to bind. We also report event-study specifications that un-pool that average effect from model 2 into period-by-period estimates that map out the dynamic path of treatment. To ensure these results (both dynamic and pooled) are generated with a panel that is balanced in event time, we limit the data to the 36 months before and after treatment before estimating model 2.

Panel B of figure 6 gives formal event study estimates of the variation on display in the raw trends. In this figure, the x-axis denotes event time. The y-axis denotes event study coefficients, with whiskers displaying 95 percent confidence intervals. The vertical dashed line denotes the reference period, which we set to the month prior to the fiscal year end date. The figure does not reveal any statistically significant evidence differences between treated and control municipalities prior to the implementation of SB5. This is true both for the earliest event study coefficients, which are estimated prior to Ferguson, and for those occurring after the events of Ferguson, in the immediate run-up to the implementation of SB5. However, despite their imprecision, there does appear to be some evidence that the post-SB5 coefficients tend to systematically lie above the x-axis. This suggests that the pooled approach from model 2 may yield more power.

To improve power, in table 3 we also report average effects that pool across all periods. Panel (a) reports estimates for all accidents. Panel (b) restricts to accidents that occurred on the interstate. Here, we find some evidence of modest changes in safety. Most notably, the number of vehicles involved in accidents each month rises by 0.515, which is moderate relative to the control mean of 3.03, and is statistically significant at conventional levels. This pattern is echoed by results for the overall number of accidents (0.228, relative to a control mean of 1.629) and the number of disabling injuries (0.03 relative to a control mean of 0.041). And while this last coefficient is large, we caution that disabling injuries are a rare outcome.

We next attempt to address the fact that our baseline specification incorporates some "forbidden comparisons" as a result of the differential treatment timing (Roth et al., 2023; Baker et al., 2025). In practice, there are two reasons this potential concern is less consequential in our specific application. First, we note that the length of time between treatment cohorts is minimal, spanning at most 12 months, and hence the "yet to be treated" can be compared to the "already" treated for, at most, 12 of the 72 coefficients on display. Second, we note that the size of the control group is 10 times larger than the size of the treatment group which means that, in prac-

tice, the vast majority of the variation used to identify model 2 is not impacted by this concern. However, out of an abundance of caution, in appendix figure A.5 and appendix table A.2 we also report specifications estimated entirely within fiscal year end-date cohorts, which eliminates the staggered treatment problem in the spirit of stacking as in Gardner (2022). As shown, the results are similar.

Thus, taken together, we argue that evidence in this section is most consistent with the idea that the cap on retained revenue had had modest impact on public safety. This is consistent with the marginal ticket being motivated by revenue, rather than safety, in municipalities over the 20 percent threshold at baseline. Having established this, we now turn to the implications of the retained revenue cap for racial equity.

8.2 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 the racial implications of policing for profit. For example, the DOJ investigative report cited anecdotal evidence of racist statements by local police and city officials in support of its claim that revenue incentives were a contributing factor to racial disparities in Ferguson (DOJ, 2015). Likewise, proponents of Sb5, which limited revenues from policing, noted that "...bringing in revenue via fines and fees comes primarily at the expense of Black citizens" (Better Together, 2014). Indeed, while Black Americans comprised 11 percent of the state population, they represented 17 percent of stops, 27 percent of searches, and 27 percent of arrests following traffic stops during 2012, our baseline year.[26] Given these patterns and motivations, reforms around policing for profit might disproportionately benefit black drivers in Missouri via an improvement in racial disparities.

To explore the racial implications of Sb5, we begin by examining whether the composition of stops changed following the change in revenue incentives associated with the introduction of SB5. To do so, we exploit the fact that our traffic stops data include information on stops by race. More specifically, we decompose the overall reduction in stops per capita of 0.358, as shown in Table 2, into stops of black drivers per capita and white drivers per capita. As shown in columns 5 and 6 of Table A.5, the reduction in stops per capita documented previously appears to be entirely explained by a reduction in stops of white drivers, with a reduction of 0.361 stops per capita, a coefficient that is nearly identical to our baseline reduction in stops per capita across all races of 0.358 in Table 2. For stops of black drivers, contrast, we detect neither an increase or a decrease following the implementation of Sb5 in treated municipalities.

[26] https://ago.mo.gov/get-help/vehicle-stops-report/2012-executive-summary/

This finding, the reduction in stops following the implementation of Sb5 being entirely explained by stops of white drivers, is consistent with a model in which police are targeting white drivers for revenue reasons and targeting black drivers for reasons other than revenue. It could be, for example, that white drivers are of higher income, leading to higher rates of collections from traffic tickets in municipal court systems. When incentives around revenue disappear, police then respond by stopping fewer of these white drivers, with no changes in stops of black drivers, who are stopped for reasons other than revenue.

These findings suggest that, if anything white drivers benefited from the reduction in stops following Sb5. Black drivers, by contrast, experienced no reduction in stops. This is despite the fact that proponents of Sb5 pointed to preexisting racial disparities between black drivers and white drivers in their interactions with the criminal justice system. Instead, given that black drivers are stopped at higher rates at baseline in Missouri, Sb5, by reducing stops of white drivers, might have had the unintended consequence of exacerbating pre-existing inequalities in policing by race.

9 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 both collections and stops. We argue our results cannot be explained by any post-Ferguson increase 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. We then explore two implications of the documented reduction in policing for profit. First, we find no corresponding reduction in safety, providing further evidence in favor of a revenue motive over a safety motive. Second, despite the fact that the Sb5 was motivated by racial concerns around excess policing of black drivers, we find that the reform led to a reduction of stops for white drivers but not for black drivers. 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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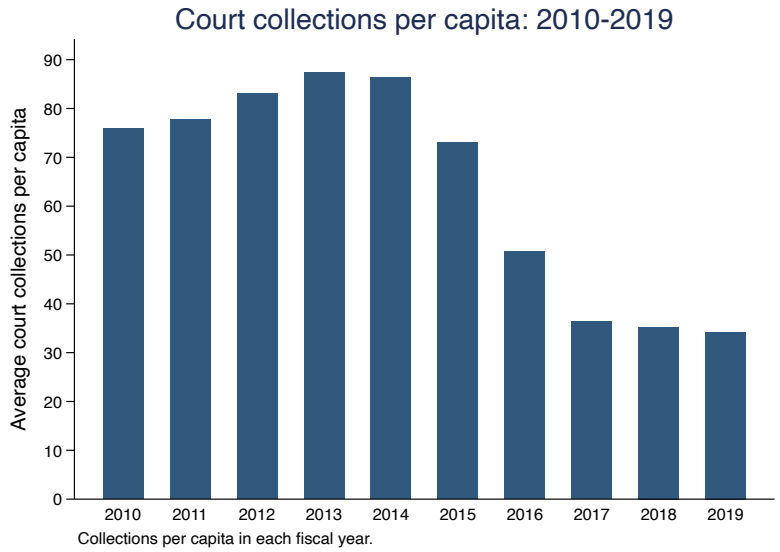
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Table 1: Summary Statistics

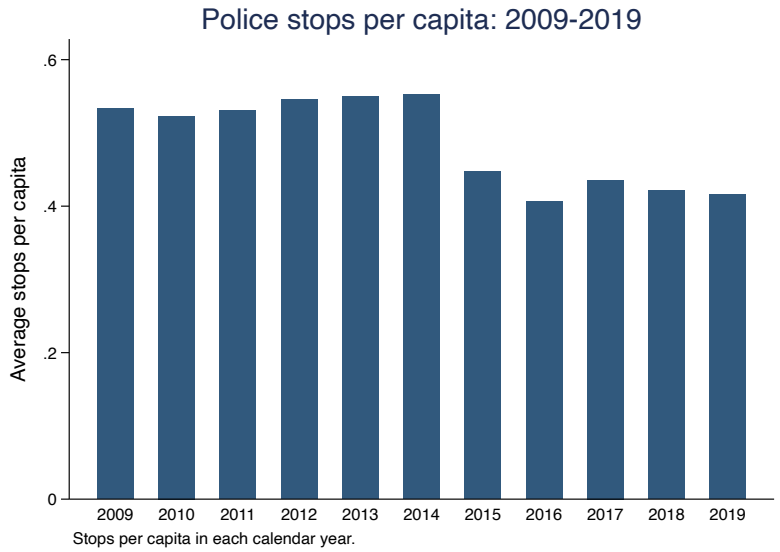
	Panel A: Demographics and Geography			
	All	Analysis	Control	Treat
Population (2010)	7,922	9,498	10,826	1,715
White share	84.8%	83.0%	85.5%	68.9%
Black share	9.8%	11.0%	8.4%	25.6%
Other race share	5.5%	6.0%	6.1%	5.5%
Interstate ratio	0.158	0.178	0.160	0.282
Any Interstate	29.4%	33.3%	33.2%	33.9%
St. Louis County/City	17.7%	19.8%	16.2%	41.1%
	Panel B: Fines and Fees			
	All	Analysis	Control	Treat
Exceed SB5 20% Threshold	14.6%	14.6%	0.0%	100.0%
Total collections per capita	83.027	68.760	38.463	246.214
Collections per capita quartile = 4	25.1%	27.6%	18.0%	83.9%
	Panel C: Policing Outcomes			
	All	Analysis	Control	Treat
Stops per capita	0.587	0.519	0.342	1.617
Stops per capita (White)	0.484	0.431	0.292	1.292
Stops per capita (Black)	0.081	0.070	0.038	0.272
Citations per capita	0.355	0.307	0.176	1.121
Warnings per capita	0.236	0.216	0.165	0.532
Municipalities	470	384	328	56

Notes: This table provides summary statistics for key variables from our data. The column labeled "All" contains the universe of municipalities in Missouri (except Ferguson) that ever had an active municipal court. We define a municipality as having an active municipal court if it collects at least one municipal fine over our sample period. The columns labeled "Control" and "Treat" are restricted to the subsample of municipalities in the "All" column that are approximately balanced, year-over-year, in terms of the existence of fines and fees, specifically, those municipalities where we can observe fines in every year for the 4 years pre-post SB5. The "Control" column contains municipalities that were below the SB5 20% threshold in 2012. The "Treat" column contains municipalities that were above the SB5 20% threshold in 2012. Unless otherwise, indicated, all summary statistics are calculated during calendar year 2012 and hence represent cross-sectional averages across municipalities. Panel A includes demographic variables from the ACS (e.g. total population, share white, share black, share other race) along with geographic variables (e.g. the ratio of interstates to land area, the presence of any interstate, whether the municipality is located inside of St. Louis city or St. Louis county). Panel B contains variables constructed from the court data, including total collections per capita, fines per capita, an indicator for whether the municipality was above the SB5 20% threshold in 2012, and an indicator for being in the fourth (highest) quartile of the distribution of fines in 2012. Panel C contains variables constructed from stop records, including the number of traffic stops per capita (in total, and separately for black/white drivers), traffic citations per capita, and traffic warnings per capita. The sample in the "All" column is primarily used to explore statewide contrasts in Missouri not involving SB5 (e.g. figure 1, 2, and 3). The Analysis "column" contains the subset of column (1) used to explore questions related to SB5, with thee specific treat-control contrast (i.e. above/below the 20% threshold) further delineated across columns (3) and (4).

Figure 1: Collections and traffic stops declined after Ferguson



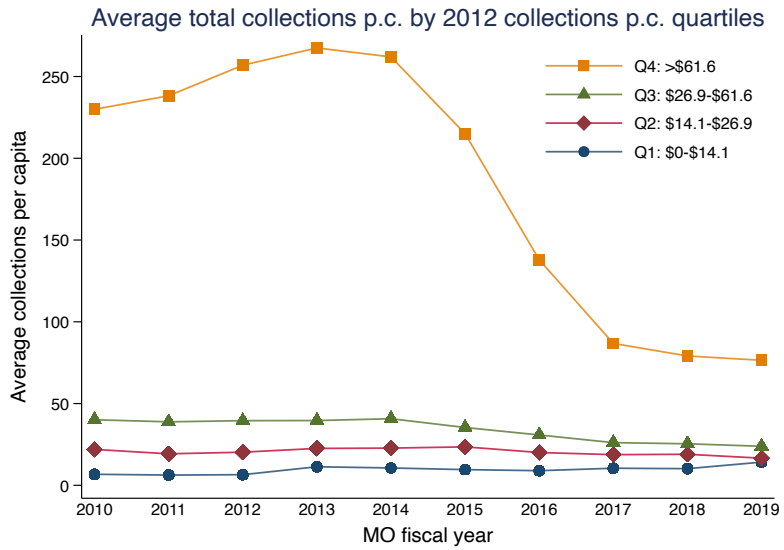
(a) Collections per capita by year



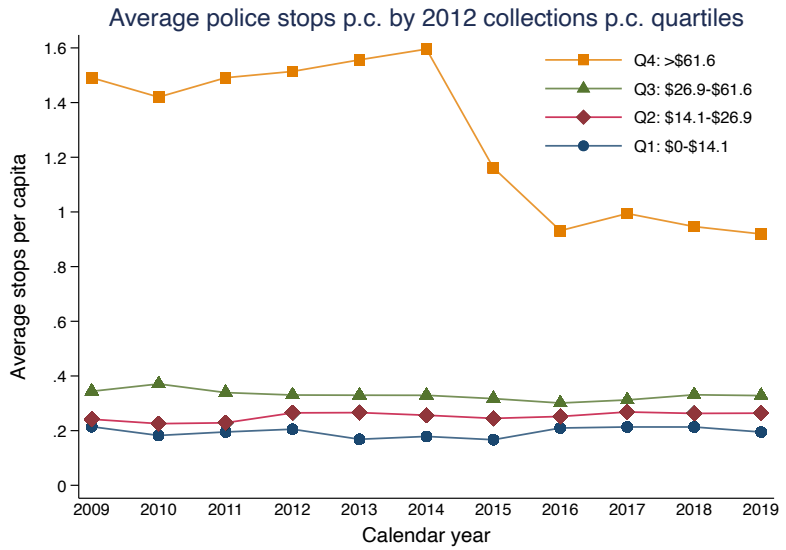
(b) Stops per resident by year

Notes: In this exhibit, we plot statewide trends for the average municipality in Missouri for two key outcome variables: Court collections per-capita (Panel A) and traffic stops per resident (Panel B). Note that the x-axis of panel A is represented in "fiscal years" to better align with the court collections data, while the X-axis of panel B is represented in calendar years to better align with the traffic stop data. Both time-series exhibit significant declines (57% for collections, 26% for stops) after the killing of Michael Brown in 2014 (fiscal year 2015).

Figure 2: Declines in collections and stops were concentrated in "high-fine" municipalities



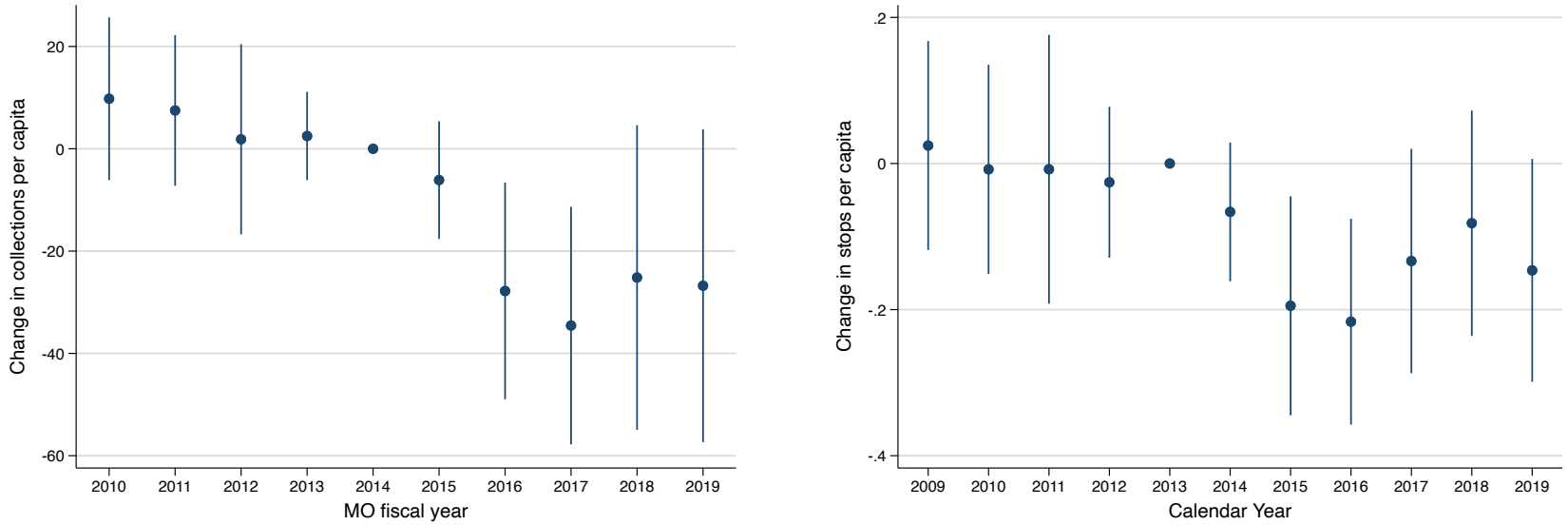
(a) Collections per capita



(b) Stops per resident

Notes: In this exhibit, we provide evidence that the statewide declines in court collections and traffic stops documented in figure 1 occurred almost exclusively in municipalities that were raising excessive revenue at baseline (and hence were more likely to be engaged in policing for profit). Specifically, we divide our sample of municipalities into four quartiles based on the distribution of fines per-capita in fiscal year 2012. Note that the x-axis of panel A is represented in "fiscal years" to better align with the court collections data, while the X-axis of panel B is represented in calendar years to better align with the traffic stop data. For the lowest three quartiles (Q1-Q3 in the figures), we see little evidence of declines in either court collections or traffic stops. However, in the highest quartile (Q4), we see large declines in the aftermath of the killing of Michael Brown in 2014 (fiscal year 2015).

Figure 3: Interstate highway traffic also predicts relative declines in collections and stops

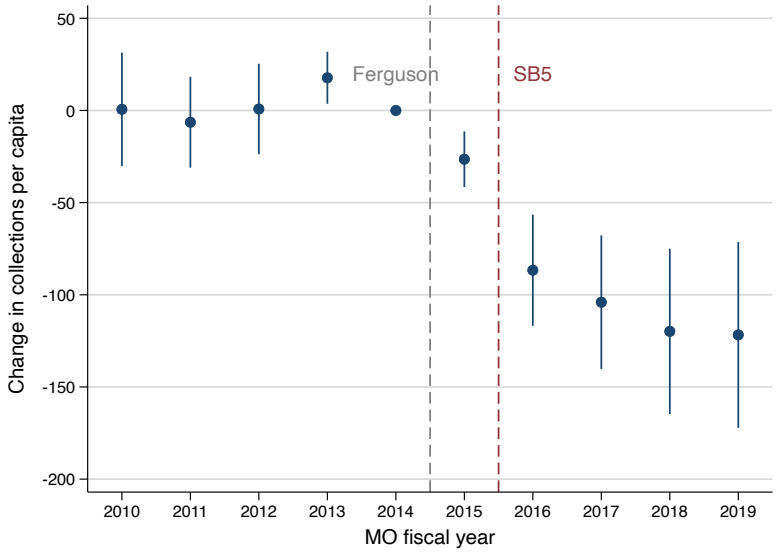


(a) Collections per capita

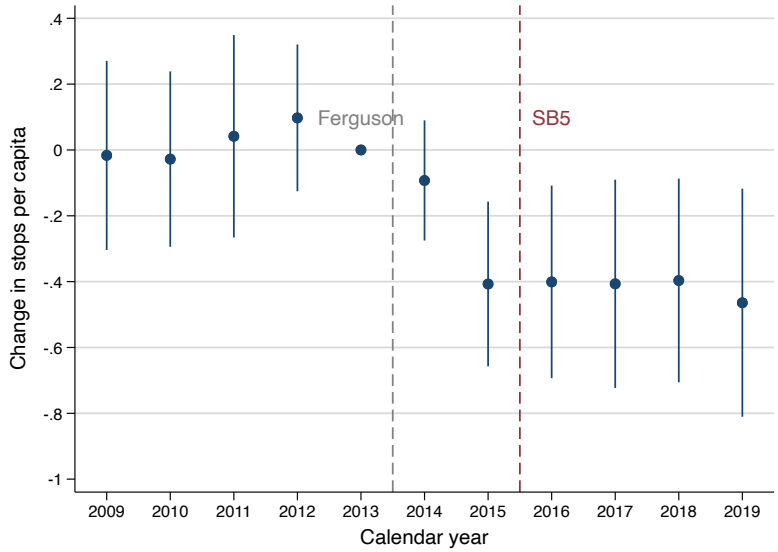
(b) Stops per resident

Notes: In this exhibit, we provide evidence that the statewide declines in court collections and traffic stops documented in figure 1 occurred disproportionately in municipalities that had motive and opportunity to engage in policing for profit via a high volume of interstate highway traffic. Specifically, we use the universe of municipalities in Missouri with an active municipal court (e.g. the sample in the "All" column of table 1) to estimate models of the form: $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times X_m + \varepsilon_{mt}$ where Y_{mt} is either court collections per capita or stops per resident in municipality m in calendar (or fiscal) year t; α_m is a municipality fixed effect; $s = s(m)$ denotes whether municipality m was in the St. Louis metro area (defined to be St. Louis City plus municipalities in St. Louis County); $\delta_{ts(m)}$ is a calendar (fiscal) year by St. Louis area fixed effect; $D_\tau = 1(t - t^* = \tau)$ is an indicator that takes a value of one τ years after the killing of Michael Brown (where t^* = calendar year 2013 for stops, and t^* = fiscal year 2014 for collections); and X_m measures the ratio of interstate highway miles to square miles of land area in the municipality–see Appendix figure A.1 for maps that show interstate penetration. Panel A gives results for court collections; Panel B gives results for traffic stops. Note that the x-axis of panel A is represented in "fiscal years" to better align with the court collections data, while the X-axis of panel B is represented in calendar years to better align with the traffic stop data. The Y-axis in each panel gives regression coefficients (β_τ) from the model described above, with associated 95% confidence intervals constructed using standard errors that are clustered at the municipality level. We find similar results using alternative measures of interstate highway penetration (see Appendix figure A.2). These findings are consistent with policing for profit via a "tax-exporting" motive as described in section 3.

Figure 4: Reductions in court collections from SB5 (20% threshold on retained revenue)



(a) Collections per capita



(b) Stops per resident

Notes: In this exhibit, we provide evidence that the SB5 20% threshold on retained revenue reduced court collections and stops. Specifically, we use the sample of municipalities contained in the "Treat" and "Control" columns of table 1 to estimate models of the form: $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times 1_{m \in M^*} + \varepsilon_{mt}$ where Y_{mt} is either court collections per capita or stops per resident in municipality m in calendar (or fiscal) year t; α_m is a municipality fixed effect; $s = s(m)$ denotes whether municipality m was in the St. Louis metro area (defined to be St. Louis City plus municipalities in St. Louis County); $\delta_{ts(m)}$ is a calendar (fiscal) year by St. Louis area fixed effect; $D_\tau = 1(t - t^* = \tau)$ is an indicator that takes a value of one τ years after the killing of Michael Brown (where t^* = calendar year 2013 for stops, and t^* = fiscal year 2014 for collections); M^* denotes the set of municipalities that were above the SB5 20% cap on retained revenue at baseline (in 2012); and $1_{m \in M^*}$ is an indicator variable that takes a value of 1 for all municipalities who were above the SB5 20% cap on retained revenue at baseline. Panel A gives results for court collections; Panel B gives results for traffic stops. Note that the x-axis of panel A is represented in "fiscal years" to better align with the court collections data, while the X-axis of panel B is represented in calendar years to better align with the traffic stop data. The Y-axis in each panel gives regression coefficients (β_τ) from the model described above, with associated 95% confidence intervals constructed using standard errors that are clustered at the municipality level. We find larger relative declines in collections and stops in municipalities that were subject to the SB5 20% limit on retained revenue.

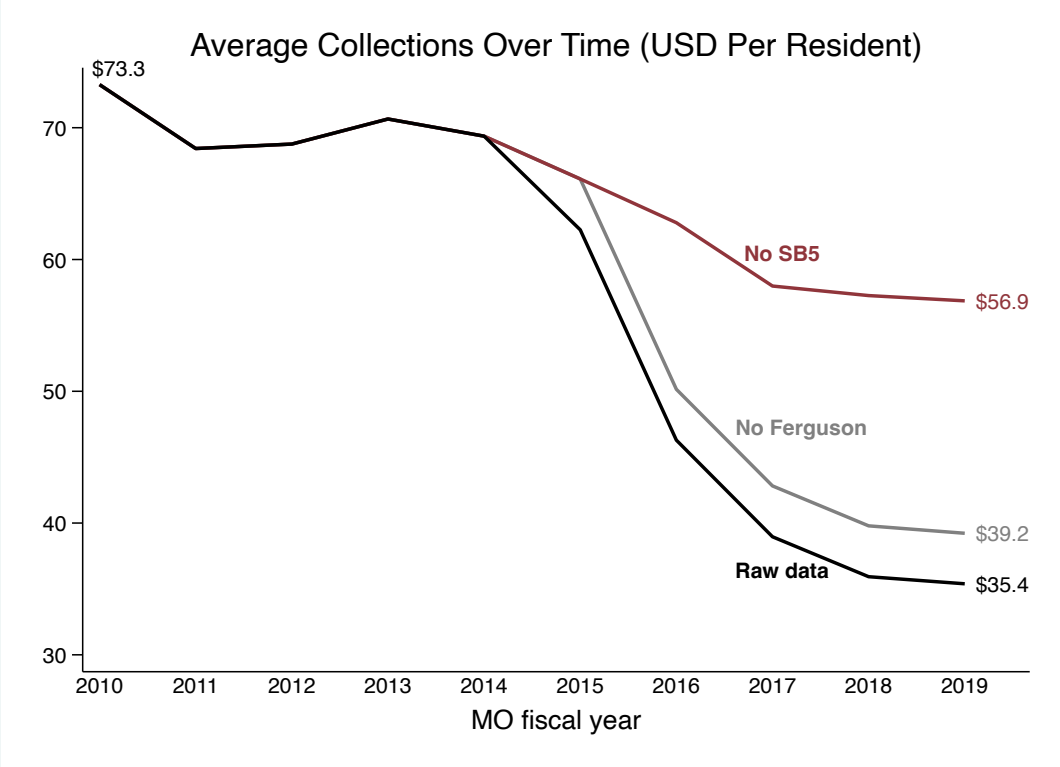
Table 2: Examining the 20% Cap on Municipal Revenue from Fines and Fees

	Panel A: Alternative Samples				
	(1)	(2)	(3)	(4)	(5)
Collections	-105.400***	-147.355***	-82.013***	-134.602***	-105.278***
	(18.850)	(33.612)	(22.139)	(42.120)	(22.057)
Stops	-0.358***	-0.216	-0.405***	-0.385**	-0.329**
	(0.101)	(0.312)	(0.087)	(0.151)	(0.162)
Sample	All	Inside STL	Outside STL	Interstate	4th Quartile
$\bar{Y}_{collections}$	57.099	100.829	46.202	71.044	145.336
$\bar{Y}_{stops}$	0.466	0.596	0.445	0.531	1.083
Municipalities	384	76	308	128	106

	Panel B: Alternative Specifications			
	(1)	(2)	(3)	(4)
Collections	-105.400***	-102.014***	-77.573***	-100.888***
	(18.850)	(18.929)	(15.222)	(18.405)
Stops	-0.358***	-0.387***	-0.358***	-0.367***
	(0.101)	(0.108)	(0.096)	(0.104)
Municipality FEs	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
St. Louis × Time FEs	Yes	Yes	Yes	Yes
Share black × Time FEs	No	Yes	No	No
Warrants × Time FEs	No	No	Yes	No
Law-suits × Time FEs	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.

Figure 5: Ferguson effect versus SB5



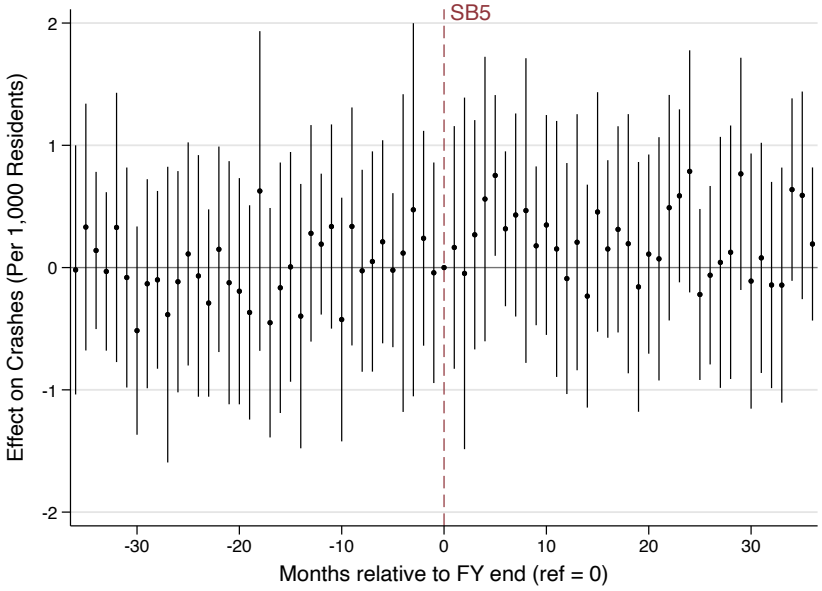
Notes: In this exhibit, we use the SB5 event study model to construct implied statewide counterfactual court collections under different scenarios. The X-axis of this figure represents fiscal years. The Y-axis is per-capita court collections. The black line ("Raw Data") plots court collections over time for the average municipality in our estimation sample using the raw data. The red line ("No SB5") plots court collections over time in our estimation sample for a counterfactual where SB5 never occurred, which we construct by setting the SB5 treatment indicator equal to zero in the event study model and then averaging across the implied values. Thus, the difference between the black and the red lines yields the implied statewide SB5 treatment effect at each point in time. Finally, to better understand how the events of Ferguson might interact with these results, we construct a second counterfactual where we assume that the entirety of the drop in collections that occurred in 2015 (after Ferguson, but before SB5) represents a persistent, differential decline caused by Ferguson. We implement this by subtracting the SB5-by-2015 coefficient in the event study model for each municipality over years 2015-2019. We then average these implied values and plot them in gray ("No Ferguson"). A comparison of the "No SB5" and "Raw Data" lines reveals that the statewide magnitude of the SB5 effect is large, explaining 50% of the total decline over this period. Comparing the "No Ferguson" to the "No SB5" lines, we see that even if we account for killing of Michael Brown by conservatively classifying the entirety of the 2015 decline as a persistent drop due to the events of Ferguson, SB5 still explains 46% of the overall decline.

Figure 6: SB5 and safety outcomes – visual evidence

(a) Raw crash trends (St. Louis metro area)



(b) Event study (all municipalities)



Notes: This figure presents evidence of the impact of SB5 on safety related outcomes. Panel (a) displays raw trends for the St. Louis area. Each dot represents crashes per 1,000 residents, measured monthly, and averaged across municipalities contained in the union of St. Louis County and St. Louis City. Grey dots represent control municipalities. Blue dots represent treated municipalities. The vertical red line denotes the month of Michael Brown's killing. The vertical black line denotes June of 2016, the most common fiscal year end date within this set of municipalities. Lines are fit to the binned averages separately for treatment and control municipalities within each segment. Panel (b) presents event study coefficients from $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_{m\tau} + \varepsilon_{mt}$ where Y_{mt} measures crashes per 1,000 residents on date t (month-by-year) in municipality m; α_m is a municipality fixed effect; $s = s(m)$ denotes whether muncipality m was in the St. Louis metro area; $\delta_{ts(m)}$ is a date by St. Louis county fixed effect; and $D_{m\tau} = 1(t - t_m^* = \tau)$ is an event study indicator that takes a value of one exactly τ months after the end of the relevant fiscal year (t_m^*) in municipality m. Standard errors are robust to heteroskedasticity. Vertical lines denote 95% confidence intervals.

Table 3: SB5 and Safety

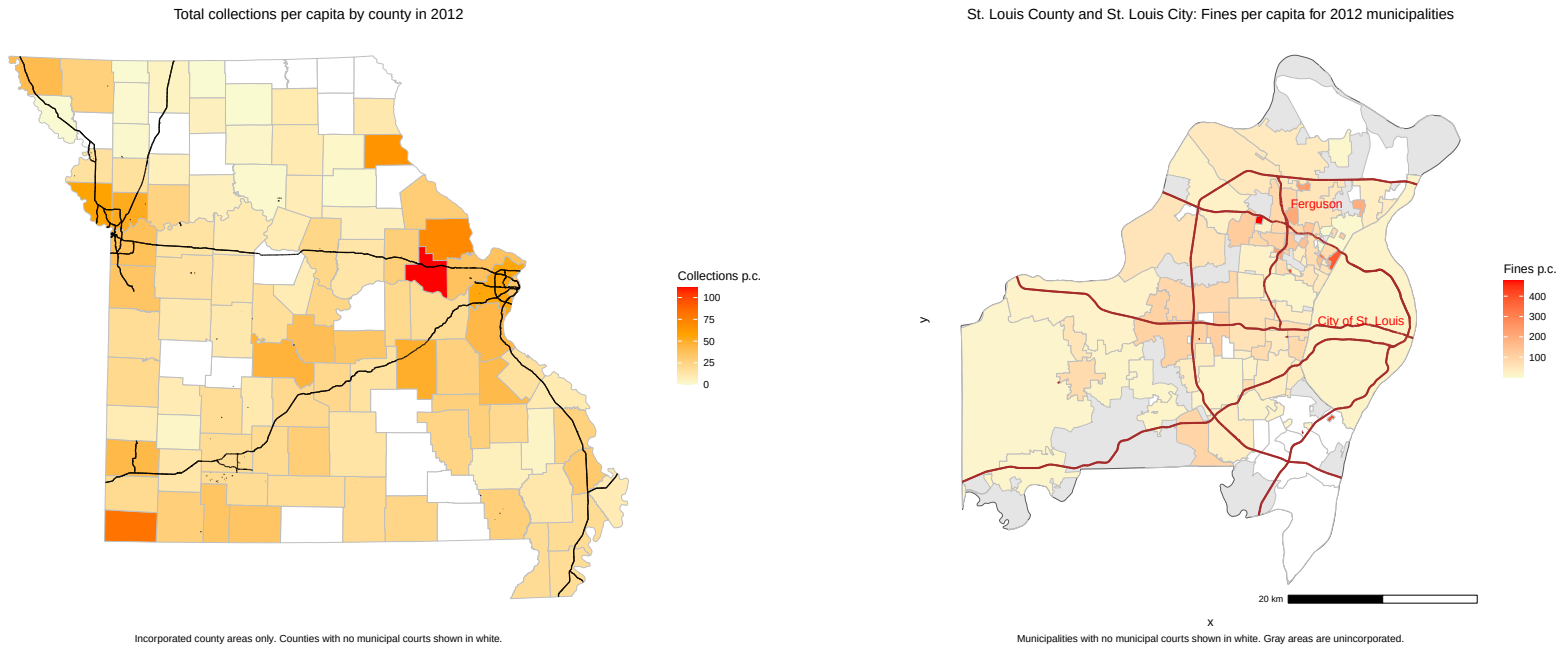
	Panel A: All Roads (Outcome per 1,000 Residents)				
	(1)	(2)	(3)	(4)	(5)
Treated X Post SB5	0.228*	0.515**	0.124	0.095	0.030***
	(0.131)	(0.258)	(0.096)	(0.090)	(0.010)
Outcome	Crashes	Vehicles	Injuries	Minor Inj.	Disabling Inj.
Control mean	1.629	3.033	0.501	0.461	0.041
	Panel B: Interstates Only (Outcome per 1,000 Residents)				
	(1)	(2)	(3)	(4)	(5)
Treated X Post SB5	-0.028	0.009	0.028	0.018	0.011
	(0.029)	(0.056)	(0.029)	(0.026)	(0.007)
Outcome	Crashes	Vehicles	Injuries	Minor Inj.	Disabling Inj.
Control mean	0.210	0.390	0.066	0.061	0.005

Notes: This table presents results for SB5 and safety. Each number is generated from a regression of the form $Y_{mt} = \alpha_m + \delta_{ts(m)} + \beta D_{mt} \times 1_{m \in M^*} + \varepsilon_{mt}$ where Y_{mt} is an outcome variable (e.g. traffic accidents), measured per 1,000 residents on date t (month-by-year) in municipality m; α_m is a municipality fixed effect; $s = s(m)$ denotes whether municipality m was in the St. Louis metro area (defined to be St. Louis City plus municipalities in St. Louis County); $\delta_{ts(m)}$ is a date by St. Louis county fixed effect; $D_{mt} = 1(t - t_m^* > 0)$ is an an indicator that takes a value of for every month after the end of the fiscal year prior to SB5 going into effect (t_m^*) in municipality m; and $1_{m \in M^*}$ is an indicator that takes a value of 1 if municipality m was treated ($m \in M^*$) as a result of being over the 20% threshold at baseline. Panel (a) gives results for all crashes. Panel (b) gives results for crashes that specifically occurred on an interstate. Within each panel, column (1) denotes all types of accidents. Column (2) denotes the total number of vehicles involved in accidents. Column (3) denotes the total number of injuries. Column (4) denotes the total number of minor injuries. Column (5) denotes the total number of disabling injuries. Corresponding event study results for the all crashes outcome is given in figure 6. In figure 6 we also display raw trends for all crashes in the St. Louis metro area, which is a convenient sample of municipalities that largely share the same treatment timing 6. Standard errors are robust to heteroskedasticity and displayed in parentheses, with stars denoting statistical significance at the 1 (***), 5(**), and 10% (*) levels.

A Online Appendix

A.1 Additional Figures and Tables

Figure A.1: Interstate highways and collections in 2012

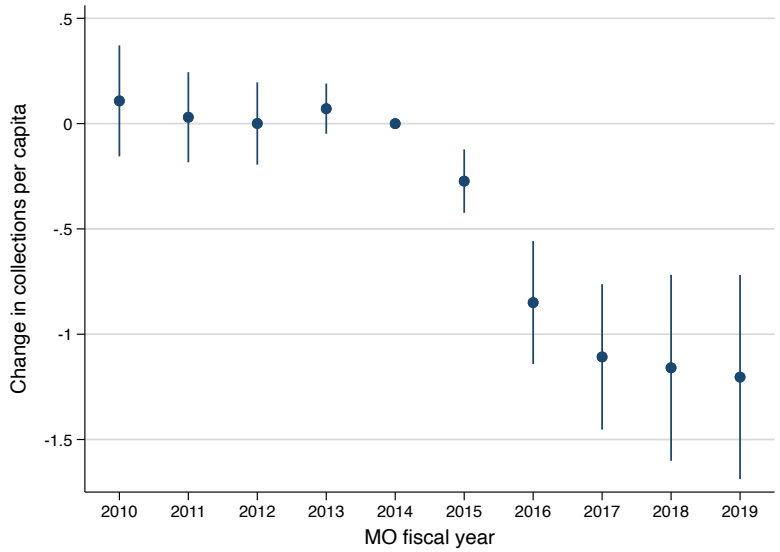


(a) Statewide map

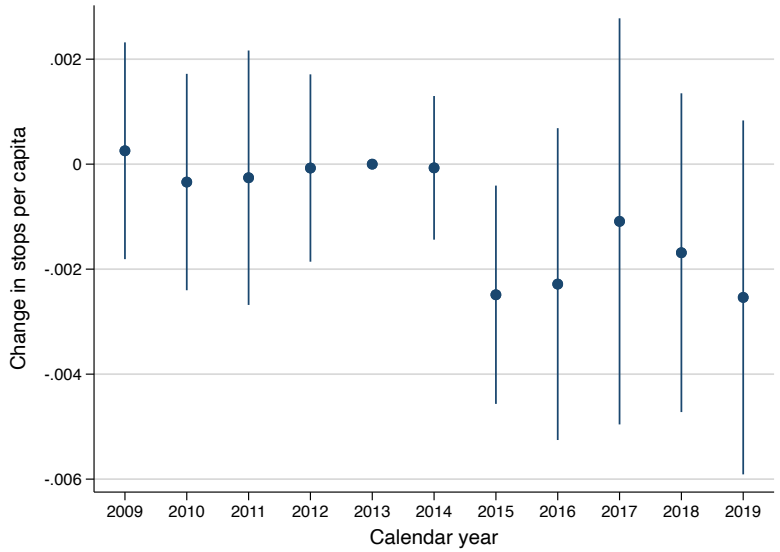
(b) St. Louis map

Notes: In this exhibit, we plot the relationship between interstate highways and collections. Panel A shows all of Missouri, with polygons representing counties. Panel B shows the St. Louis Metro Area (city and county), with polygons representing specific municipalities. The shading denotes collections per-capita inside the indicated polygon. Dark lines represent interstate highways. Visually, it is apparent that collections per-capita are higher in places with a high-concentration of interstates. This is consistent with the anecdotal evidence (discussed in section 3 that interstate highways create both motive and opportunity to police for profit.

Figure A.2: Interstate highway penetration also predicts relative declines in collections and stops



(a) Collections per capita



(b) Stops per resident

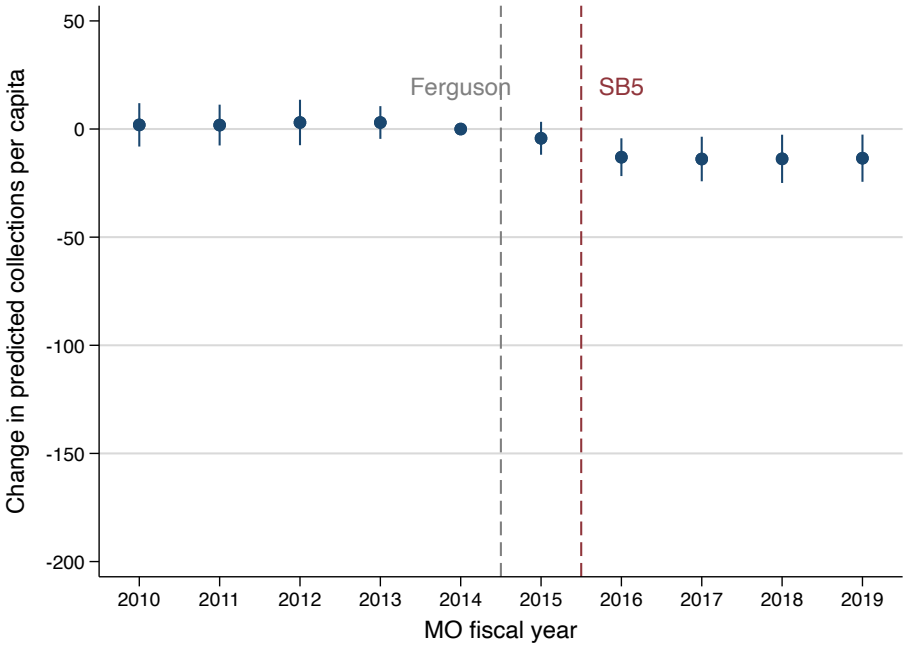
Notes: In this exhibit, we provide evidence that the statewide declines in court collections and traffic stops documented in figure 1 occurred disproportionately in municipalities that had motive and opportunity to engage in policing for profit via the existence of extensive interstate highway networks. Specifically, we use the universe of municipalities in Missouri with an active municipal court (e.g. the sample in the "All" column of table 1) to estimate models of the form: $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times X_m + \varepsilon_{mt}$ where Y_{mt} is either court collections per capita or stops per resident in municipality m in calendar (or fiscal) year t; α_m is a municipality fixed effect; $s = s(m)$ denotes whether municipality m was in the St. Louis metro area (defined to be St. Louis City plus municipalities in St. Louis County); $\delta_{ts(m)}$ is a calendar (fiscal) year by St. Louis area fixed effect; $D_\tau = 1(t - t^* = \tau)$ is an indicator that takes a value of one τ years after the killing of Michael Brown (where t^* = calendar year 2013 for stops, and t^* = fiscal year 2014 for collections); and X_m measures the maximum average annual daily traffic across all highway segments in the municipality. Panel A gives results for court collections; Panel B gives results for traffic stops. Note that the x-axis of panel A is represented in "fiscal years" to better align with the court collections data, while the X-axis of panel B is represented in calendar years to better align with the traffic stop data. The Y-axis in each panel gives regression coefficients (β_τ) from the model descirbed above, with associated 95% confidence intervals constructed using standard errors that are clustered at the municipality level.

Table A.1: Collections and Stops and SB5

	Panel A: Collections per capita			
	(1)	(2)	(3)	(4)
Treated X Post SB5	-105.400***		-107.029***	-97.826***
	(18.850)		(19.640)	(22.035)
Treated Cts X Post SB5		-347.281***		
		(106.460)		
Observations	3805	3805	3556	3444
Clusters	384	384	359	347
Adjusted R2	0.85	0.85	0.83	0.86
	Panel B: Stops per capita			
	(1)	(2)	(3)	(4)
Treated X Post SB5	-0.358***		-0.335***	-0.289**
	(0.101)		(0.120)	(0.117)
Treated Cts X Post SB5		-2.374***		
		(0.857)		
Observations	4002	4002	3695	3612
Clusters	370	370	342	332
Adjusted R2	0.82	0.82	0.80	0.83

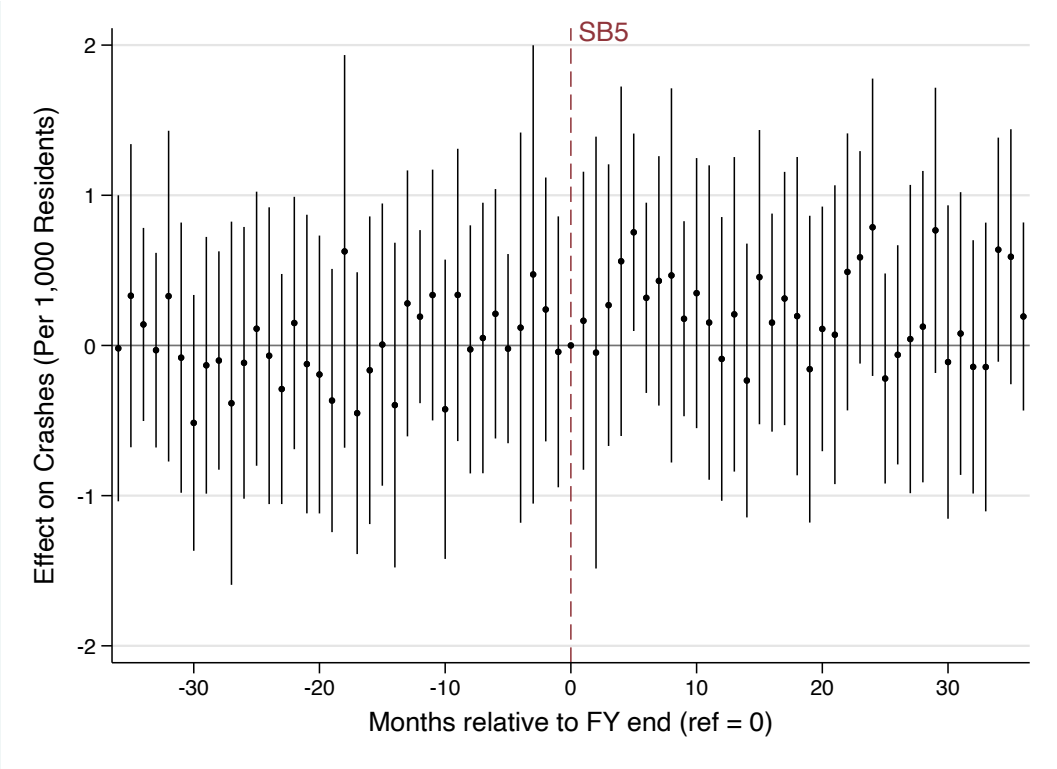
Notes: All specifications include municipality and year FE, and St. Louis by year FE. Standard errors clustered by place. Column 1 shows baseline results. Column 2 uses a continuous treatment. Column 3 includes county X year FE. Column 4 drops cities with pop<500."

Figure A.3: Ferguson effect versus SB5



Notes: This figure reports event-study estimates using predicted collections per capita as the outcome. We first estimate the following regression using only the pre-Ferguson sample $C_{mt} = \alpha_m + \delta_{ts(m)} + \gamma S_{m,t-1} + \varepsilon_{mt}$, where C_{mt} denotes fiscal-year court collections per capita in municipality m and year t, $S_{m,t-1}$ denotes stops per capita in the prior calendar year, α_m is a municipality fixed effect, and $\delta_{ts(m)}$ is a fiscal-year by St. Louis area fixed effect. We then use the fitted values from this regression, $\widehat{C}_{mt}^{\text{pred}} = \hat{\alpha}_m + \hat{\delta}_{ts(m)} + \hat{\gamma} S_{m,t-1}$, to construct predicted collections per capita for all municipality-year observations. Finally, we estimate equation (1) using $\widehat{C}_{mt}^{\text{pred}}$ as the dependent variable. The figure plots the resulting event-study coefficients and corresponding 95 percent confidence intervals, with standard errors clustered at the municipality level. We use lagged stops because the fiscal year ends before the calendar year and because stops can generate future collections, but not vice versa.

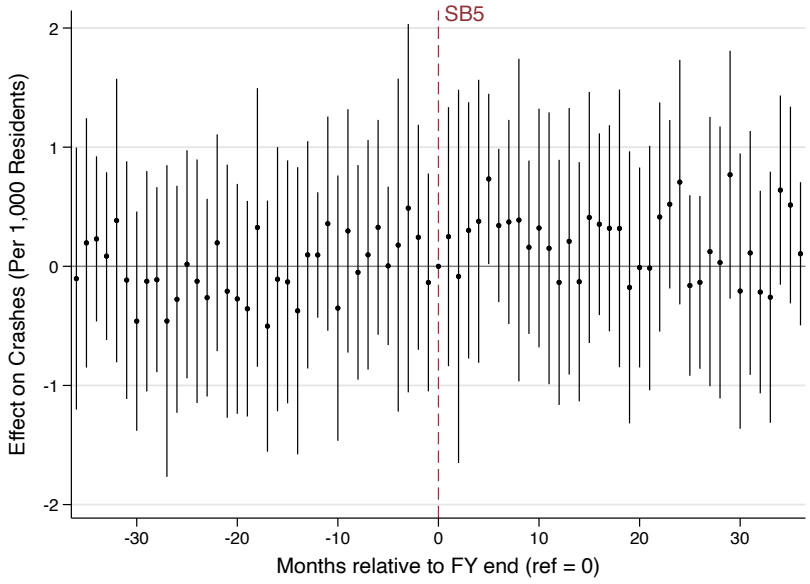
Figure A.4: SB5 and Safety – Event Study (Interstates Only)



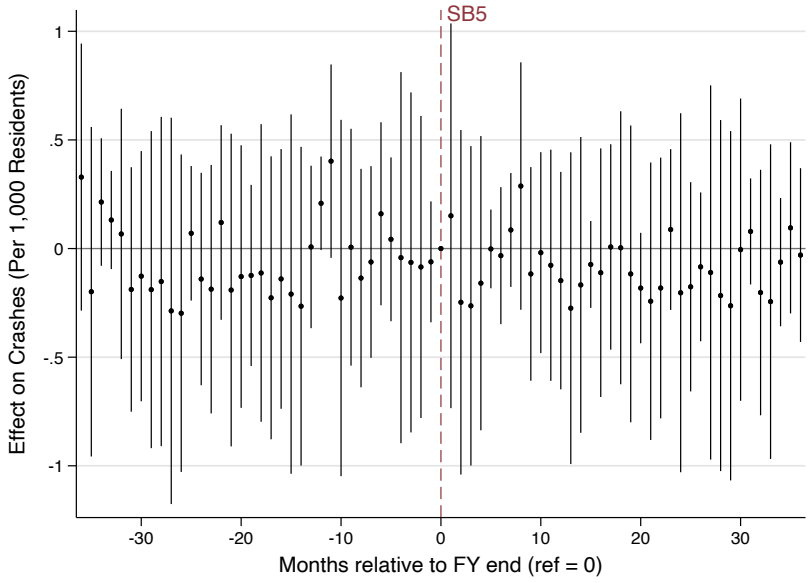
Notes: This figure presents event study coefficients from $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_{m\tau} + \varepsilon_{mt}$ where Y_{mt} measures interstate crashes per 1,000 residents on date t (month-by-year) in municipality m; α_m is a municipality fixed effect; $s = s(m)$ denotes whether muncipality m was in the St. Louis metro area (defined to be St. Louis City plus municipalities in St. Louis County); $\delta_{ts(m)}$ is a date by St. Louis county fixed effect; and $D_{m\tau} = 1(t - t_m^* = \tau)$ is an event study indicator that takes a value of one exactly τ months after the end of the 2016 fiscal year (t_m^*) in municipality m. In effect, this figure is generated identically to panel (b) of figure 6, except that we restrict the outcome variable to only count crashes that occurred on the interstate. Standard errors are robust to heteroskedasticity. Vertical lines denote 95% confidence intervals.

Figure A.5: Robustness: Stacked Event Study

(a) All Crashes



(b) Interstate Crashes



Notes: This figure presents event study coefficients from $Y_{mt} = \alpha_m + \delta_{ts(m)c(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_{m\tau} + \varepsilon_{mt}$ where Y_{mt} measures crashes per 1,000 residents on date t (month-by-year) in municipality m; α_m is a municipality fixed effect; $s = s(m)$ denotes whether muncipality m was in the St. Louis metro area (defined to be St. Louis City plus municipalities in St. Louis County); $c = c(m)$ maps municipalities into treatment cohorts based on their fiscal-year end date; $\delta_{ts(m)c(m)}$ is a date by St. Louis county by fiscal year end date cohort fixed effect; and $D_{m\tau} = 1(t - t_m^* = \tau)$ is an event study indicator that takes a value of one exactly τ months after the end of the 2016 fiscal year (t_m^*) in municipality m. Panel (a) gives results for all crashes. Panel (b) gives results for crashes that occured in on interstate. Standard errors are robust to heteroskedasticity. Vertical lines denote 95% confidence intervals.

Table A.2: Robustness: Stacked Dif-in-Dif

	Panel A: All Roads (Outcome per 1,000 Residents)				
	(1)	(2)	(3)	(4)	(5)
Treated X Post SB5	0.231*	0.521*	0.125	0.099	0.028***
	(0.135)	(0.269)	(0.101)	(0.095)	(0.010)
Outcome	Crashes	Vehicles	Injuries	Minor Inj.	Disabling Inj.
Control mean	1.633	3.040	0.502	0.461	0.040
	Panel B: Interstates Only (Outcome per 1,000 Residents)				
	(1)	(2)	(3)	(4)	(5)
Treated X Post SB5	-0.033	0.003	0.028	0.018	0.011
	(0.029)	(0.059)	(0.030)	(0.027)	(0.007)
Outcome	Crashes	Vehicles	Injuries	Minor Inj.	Disabling Inj.
Control mean	0.212	0.392	0.066	0.061	0.005

Notes: This table presents results for SB5 and safety. Each number is generated from a regression of the form $Y_{mt} = \alpha_m + \delta_{ts(m)c(m)} + \beta D_{mt} \times 1_{m \in M^*} + \varepsilon_{mt}$ where Y_{mt} is an outcome variable (e.g. traffic accidents), measured per 1,000 residents on date t (month-by-year) in municipality m; α_m is a municipality fixed effect; $s = s(m)$ denotes whether municipality m was in the St. Louis metro area (defined to be St. Louis City plus municipalities in St. Louis County); $c = c(m)$ maps municipalities into treatment cohorts based on their fiscal year end date; $\delta_{ts(m)c(m)}$ is a date by St. Louis county by fiscal year end date cohort fixed effect; $D_{mt} = 1(t - t_m^* > 0)$ is an an indicator that takes a value of for every month after the end of fiscal year 2016 (t_m^*) in municipality m; and $\mathbb{1}_{m \in M^*}$ is an indicator that takes a value of 1 if municipality m was treated ($m \in M^*$) as a result of being over the 20 percent threshold at baseline. Panel (a) gives results for all crashes. Panel (b) gives results for crashes that specifically occurred on an interstate. Within each panel, column (1) denotes all types of accidents. Column (2) denotes the total number of vehicles involved in accidents. Column (3) denotes the total number of injuries. Column (4) denotes the total number of minor injuries. Column (5) denotes the total number of disabling injuries. Standard errors are robust to heteroskedasticity and displayed in parentheses, with stars denoting statistical significance at the 1 (***), 5(**), and 10% (*) levels.

B Data Appendix

B.1 Safety data

This appendix provides additional details on the traffic accident data from the Missouri Highway patrol. The raw data consists of 10 separate spreadsheets, one each from 2009 to 2019. Within each spreadsheet, the unit of observation is a traffic accident. We load these files and append them to form a single data set spanning the entire time period and consisting of 1,636,313 reported accidents. We drop the 7,945 (0.5%) of observations missing a crash date. Next, we spatially merge the latitude and and longitude of each observation to shapefiles of 2010 municipality boundaries provided by the Census. We fail to match 466,927 of these accidents (28% of observations) to a municipality. The vast majority of these non-matching observations (337,663 or 72.3%) do not match because they happened in unincorporated areas and hence do not belong to any municipality in Missouri. From there, we aggregate the data to the municipality month level and complete the panel by adding back implicit zeros in municipalities which have no reported accidents in a given month. Finally, to construct per-capita variables, we divide by total population for year 2010 as reported in the Census. Appendix table A.3 provides summary statistics.

Table A.3: Summary Statistics: Safety Data

	Panel A: All Roads (Outcome per 1,000 Residents)			
	All	Analysis	Control	Treat
Crashes	1.842	1.406	1.319	1.914
Vehicles	3.438	2.598	2.456	3.427
Injuries	0.736	0.477	0.445	0.660
Minor Injuries	0.644	0.428	0.399	0.593
Disabling Inj.	0.093	0.049	0.046	0.067
	Panel B: Interstates Only (Outcome per 1,000 Residents)			
	All	Analysis	Control	Treat
Crashes	0.337	0.180	0.150	0.359
Vehicles	0.662	0.323	0.273	0.620
Injuries	0.094	0.056	0.048	0.097
Minor Injuries	0.089	0.051	0.044	0.091
Disabling Inj.	0.004	0.005	0.005	0.007
N	5,640	4,608	3,936	672
Municipalities	470	384	328	56

Notes: This table provides summary statistics for the accident data. Each number in this table is a mean for the relevant outcome variable reported in the row. Columns (1) corresponds to all municipalities. Column (2) corresponds to municipalities that serve as the control group for the SB5 cap on retained revenue. Column (3) corresponds to municipalities that were treated by the SB5 cap on retained revenue.

B.2 Policing Budgets and Workforce

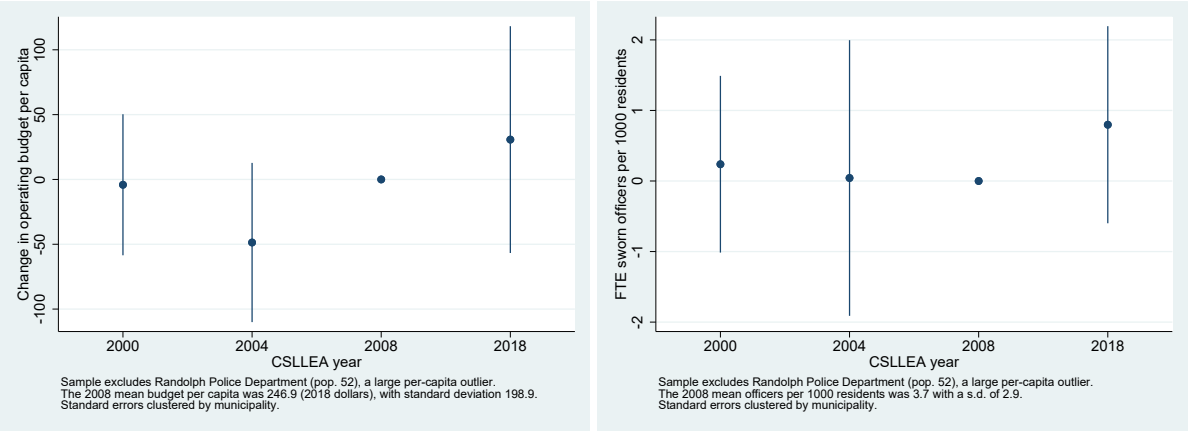
In this section we explore whether the observed decline in policing activity after SB5 could be the result of decreased funding for policing. We use data from the Census of State and Local Law Enforcement Agencies (CSLLEA) for 2000, 2004, 2008, and 2018 and focus on measures of police department operating budgets and the size of the workforce (officer counts).[27]. 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 with our SB5 treatment variable, D_i, and include municipality η_i and η_t year fixed effects.

$$Y_{it} = \sum_{k \in T} \beta_k D_i \times \mathbf{1}(t = k) + \eta_i + \eta_t + \varepsilon_{it} \tag{3}$$

[27]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

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 A.6 we show event study plots for operating budget per capita and officers per 1000 residents, using a measure of full-time equivalent sworn officers.[28] 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 A.6: Police department operating budget and officer count: event studies



(a) Operating budget p.c.

(b) Officers per 1000 residents

In Table A.4 we present results from the two-way fixed effect version of equation 3 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 A.6. 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 and officer count.[29] 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.

[28] 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.

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

Table A.4: 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.

C ADDITIONAL EXHIBITS

Table A.5: Additional Evidence

	(1) Ln coll.	(2) Ln stops	(3) Cites pc	(4) Warnings pc	(5) B stops pc	(6) W stops pc
Treated X Post SB5	-0.461***					
	(0.110)					
Treated X Post SB5		-0.168**	-0.345***	-0.041	0.005	-0.361***
		(0.073)	(0.089)	(0.045)	(0.041)	(0.082)
Observations	3697	4002	4002	4002	4002	4002
Overall mean	11.381	6.703	0.257	0.214	0.063	0.386
Control mean	11.394	6.705	0.237	0.202	0.054	0.364
Clusters	382	370	370	370	370	370
Adjusted R2	0.93	0.90	0.80	0.78	0.58	0.86

Notes: Column 1 represents the natural log of collections while column 2 represents the natural log of stops. Columns 3 and 4 decompose the reduction in stops per capita into a reduction in citations per capita and warnings per capita. Columns 5 and 6 decompose the reduction in stops per capita into a reduction in stops of black drivers per capita and white drivers per capita.