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

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Abstract

While policing is a public service, it can also be a source of 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 in Ferguson. We document large aggregate declines in per-capita collections immediately after the shooting. This decline is concentrated in municipalities with the motive and opportunity to engage in policing for profit. Consistent with changes in policing, we find corresponding declines in vehicle stops in these municipalities, and these reductions are driven by declines in stops resulting in citations, which generate revenue, rather than by warnings. The reduction in stops is attributable to white drivers, with no change for black drivers. Given that blacks are stopped at higher rates than whites at baseline, the declines in policing, if anything, exacerbated existing racial disparities. We also find a corresponding increase in crashes, suggesting that reductions in revenue-driven policing might compromise public safety. To shed further light on the objective function of city officials, we then focus on a specific reform that changed revenue incentives by limiting retained revenue from policing to 20 percent of a municipality's general revenue. Consistent with revenue-driven policing, we find that municipalities treated by the reform significantly reduced collections, and these declines cannot be explained by the more general post-Ferguson changes in policing. Taken together, our findings highlight how revenue motives can distort the criminal justice system and how simple reforms that realign 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."

— John Gwaltney, Mayor of Edmundson, Missouri, in a memo to the Edmundson Police Department

1 Introduction

In the United States, policing is an important public service provided by local governments. At the same time, 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 regula-

[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)

tory environment shape policing, as well as their downstream impact on public safety and racial disparities. Our setting is Missouri, where the shooting of Michael Brown and subsequent investigation led to both a spotlight on policing for profit 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 shifted 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 key outcome, measured on an annual basis, is per-capita municipal court revenue collections. We supplement these data with information on police traffic stops, 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 collections per-capita (57 percent) in the aftermath of the shooting. This decline was concentrated in places that were raising large amounts of per-capita revenue via fines and fees prior to Ferguson, suggesting that municipalities likely to be engaged in policing for profit were most affected. More directly, we find that the aggregate declines are also concentrated in municipalities with the incentive (via tax exporting) to engage in revenue generation: municipalities with high levels of highway traffic experience drops that are larger than average. In all cases, these large and sudden declines occur despite the fact that per-capita revenue 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.

We also find evidence of changes in police behavior that are consistent with a revenue motive. To shed light on police behavior in the aftermath of Ferguson, we incorporate data on vehicle stops. Similar to the decline in collections, we find a corresponding average decline in vehicle stops by police both in aggregate and especially in places with the motive and opportunity to en-

[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 collection of fines 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).

gage in policing for profit. One possible explanation for this decline could be a uniform reduction in effort across all law-enforcement activity in response to the outrage in Ferguson—a so-called "Ferguson Effect." If this theory were true, we would expect police stops of all types to decline; however, that is not what we find. Consistent with a revenue motive, we observe that the decline in stops is almost entirely driven by stops resulting in a citation, which generate revenue, rather than stops resulting in a warning, which do not generate revenue. This pattern is consistent with outrage over Ferguson and the reforms enacted in its aftermath causing a reduction in policing for profit.

We also find evidence that the reduction in policing for profit reduced public safety. To explore public safety, we incorporate publicly available data from the Missouri State Highway patrol covering the universe of motor vehicle accidents over this period. A key advantage of these data is that they are recorded at the incident level, allowing us to explore changes in trends at finer temporal resolutions (i.e. monthly, rather than yearly). We find a post-Ferguson increase in the average number of vehicles per month that are involved in accidents in places with the motive and opportunity to engage in policing for profit. We find similar results for other measures of traffic safety (e.g. frequency of injuries or their severity). Overall, these findings are consistent with the idea that the post-Ferguson decline in revenue also led to a deterioration in public safety.

We find evidence that white drivers disproportionately benefited from the reductions in policing for profit. Given that black Americans have more frequent contact with the criminal justice system than white Americans, the post-Ferguson narrative around policing for profit prominently highlighted the role of race. If the costs of policing for profit are disproportionately borne by Blacks, relative to whites, then ending or reducing the practice should improve racial equity. 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 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, the post-Ferguson decline in policing for profit seems to have, if anything, exacerbated these preexisting disparities between black and white drivers in Missouri.

We conclude by exploring the relative importance of policy reforms versus scrutiny as the key mechanism for our findings. Specifically, we focus on a bill known as SB5, which placed a cap on revenue retained by the municipality. More precisely, the law required that any revenue from fines and fees in excess of 20 percent of general revenue be remitted to the state, limiting the direct incentive for local municipalities to engage in policing for profit. Effectively, the law created a sharp reduction in the local revenue share of a marginal ticket, dropping from one to

zero in those places raising more than 20 percent at baseline. This change in incentives also allows us to also shed light on the objective functions of the police department and other city officials. Under a revenue motive, this change should lead to dramatic reductions in revenue from policing. Under a safety motive, by contrast, there should be no changes in revenue. Consistent with a revenue motive, we find significant relative declines in collections ($105 less per resident) in municipalities with a share of revenue from policing in excess of 20 percent at baseline. The magnitude of the relative reduction 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 could be 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.

However, a plausible alternative explanation to SB5 could be increased scrutiny to the practice of policing for profit. While the large differential decline in municipalities impacted by SB5 is consistent with the change in incentives leading to a corresponding change in police behavior, it is also potentially consistent with 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 explanations.

We argue that the cap on retained revenue is the dominant force behind the decline in collections. 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 lawsuits 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 show that the magnitude of the SB5 effect is larger than the decline that would be predicted by a post-Ferguson decline in policing. In particular, we first develop measures of revenue per stop, using pre-Ferguson data, and then show that the post-Ferguson decline in revenue predicted by the post-Ferguson decline in stops is much smaller than the decline in revenue attributable to

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

SB5. Finally, we investigate the salience of the specific 20 percent cap on retained revenue in SB5. In particular, we show that the relative decline in revenue is nearly identical to the decline in revenue that would be predicted if all treated municipalities, those collecting more than 20 percent at baseline, reduced their share of revenue from policing to exactly 20 percent, with no changes in revenue for places unaffected by the cap on retained revenue. We are not aware of any other post-Ferguson reforms that involved a 20 percent threshold, suggesting that our baseline declines in revenue are indeed shaped by the sharp change in incentives generated by SB5.

2 Related Literature

Our paper contributes to a growing, interdisciplinary literature on policing for profit by studying how policing for profit changes during a period with increased scrutiny of and reforms around this practice. 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 contribution involves examining whether policing for profit declined during a period with both increased scrutiny of and reforms designed to curb this practice. We also contribute via the examination of the 20 percent threshold on retained revenue, allowing us to shed light on the objective functions of police and other local leaders. Morevoer, our identification strategy—a differences-in-differences research design that leverages a sharp change around the events of Ferguson—is also distinct from prior work, which generally exploits cross-state panel variation in general fiscal conditions.

More broadly, our study contributes to a large literature on policing in the United States by examining how police departments respond to both increased scrutiny of and reforms around

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

policing for profit. 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 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 increased scrutiny of policing for profit, along with 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

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

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

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 curbing policing for profit 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-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 the role of highway traffic in driving policing for profit and the associated declines in revenue in these places with traffic after the shooting in Ferguson and the associated spotlight on policing for profit.

3 Background

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]

In the aftermath of Ferguson, and in the search for structural factors leading to the interaction between Michael Brown and Darren Wilson and the subsequent shooting, a spotlight was placed on the issue of over-policing in general and revenue-driven policing in particular. The U.S. Department of Justice Report into the practices of the Ferguson police department and court system specifically highlighted revenue incentives and policing for profit as contributing factors (De-

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

partment of Justice, Civil Rights Division, 2015). The report emphasized Ferguson's reliance on revenue from policing, noting that the city budgeted for significant increases in municipal fines and fees. For example, the 2012 municipal budget included a 30 percent increase in revenue from fines and fees, increasing from 1.41 million to 1.92 million dollars. Even that aggressive target was more than met, with the city ultimately collecting 2.11 million dollars in that fiscal year. According to the report, the city "exhorts police and court staff to deliver those revenue increases" and "Officer evaluations and promotions depend to an inordinate degree on 'productivity,' meaning the number of citations issued". The report concluded that "Ferguson's law enforcement practices are shaped by the City's focus on revenue rather than by public safety needs".

Importantly, revenue-driven policing in Missouri prior to the shooting was not unique to Ferguson. For example, as noted above, the Mayor of Edmundson, a small municipality close to Ferguson, wrote a memo to the Police Department in April 2014 reminding officers that revenue generated by policing "will directly affect pay adjustments at budget time". A report written by Better Together, a think tank in St. Louis, identified 21 municipalities in St. Louis County alone that raised more than 20 percent of their revenue from fines and fees in 2013, the year before the shooting. More generally, the subsequent attention to this issue after the shooting in Ferguson led to calls for curbing revenue-driven policing both locally and nationally (The Washington Post Editorial Board, 2015; The New York Times Editorial Board, 2015).

In the wake of the post-Ferguson spotlight on policing for profit, the state enacted several policies designed to curb the practice. The key piece of state legislation was Senate Bill 5 (SB5), passed in August 2015. Some provisions in the bill, such as a cap of 300 dollars on individual fines, went into effect immediately.[9] Arguably the most important provision, which took effect on January 1, 2016, limited local governments from raising more than 20 percent of municipal general revenue from traffic fines and fees.[10] Importantly, the law *did not prevent* municipalities from collecting revenue beyond 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).[11] 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 in section 7.

[9] https://finesandfeesjusticecenter.org/research-tools/missouri-sb-5-fines-fees-municipal-courts/

[10] More specifically, the law took effect on January 1 for municipalities with a fiscal year start date of January 1. For municipalities with non-calendar fiscal years, the law took effect at the start of the fiscal year that began during 2016.

[11] 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).

Beyond SB5, Supreme Court rule 37.04, which went into effect on July 1, 2017 removed the ability of Missouri courts to issue warrants and hold individuals in jail for minor traffic violations (Supreme Court of Missouri, 2016). In addition, Arch City Defenders, an advocacy group in St. Louis, filed a series of civil lawsuits against local governments in Missouri following the shooting. These suits, starting in 2015, involved revenue collected via fines and fees, with a focus on so-called "debtors' prisons". These lawsuits resulted in both settlements and reforms in targeted jurisdictions.[12]

Given the multiple reforms and the more general controversies around policing after Ferguson, our analysis begins by analyzing changes after the shooting. That is, we take the August 2014 shooting in Ferguson as the treatment date and examine responses to the subsequent spotlight on policing for profit and the associated reforms as a bundle. In order to study more directly the role of the policy reforms, and thereby shed light on the objective functions of the police and other local leaders, we then turn to a more specific analysis of SB5. In particular, we use the 20 percent limit on retained revenue from policing as the definition of treatment and the implementation of this provision during 2016 as the treatment date.

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.[13] 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.[14] 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.[15] Court statistics contained in this data include

[12] https://www.archcitydefenders.org/our-impact/debtors-prisons/

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

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

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

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.[16] Importantly, the Missouri court statistics record all of the revenue collected from court fines and fees, which includes revenue that would be retained by the municipality and 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. To measure the denominator, we use comprehensive annual financial reports (CAFRs) for each municipality from the MO auditor's office.[17] Both the numerator and the denominator are measured at baseline, prior to the shooting. In particular, 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.[18]

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.[19] 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[20] provides data

DOJ investigation.

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

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

[18]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.

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

[20]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.

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 impacts of curbing policing for profit 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 6.

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 universe of municipalities in Missouri that ever had an active municipal court over fiscal years 2010-2019.[21] We use this sample to explore statewide questions related to declining municipal revenue that occurred in the aftermath of Ferguson.

The "Analysis" column contains the subset of municipalities we use to explore questions related to policing for profit. 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.[22] We impose this restriction to avoid econometric issues related to imbalanced panels that can be particularly salient in the context of event studies.

The remaining columns of table 1 show summary statistics for three sub-samples of interest. The first subsample, labeled "Q4" in the table, contains municipalities in the top quartile of collections at baseline. We take this as a measure of municipalities that are likely to be engaged in policing for profit. The second subsample, labeled "Traffic", contains municipalities with above median highway traffic. We take highway traffic as a measure that describes which municipalities had opportunity and motive (via tax-exporting) to engage in policing for profit. The third subsample, labeled "SB5", contains municipalities expect to be impacted by the passage of SB5, which are those municipalities raising more than 20 percent of revenue from policing at baseline. This subsample will be helpful for exploring the role of policy reforms as a mechanism in section 7.

Table 1 reveals that places in the top quartile of the collections per capita distribution are

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

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

different on a number of observable characteristics. They tend to be smaller in terms of population (4,284 residents versus 9,498 across our entire analysis sample), to have a higher share of black residents (23.8 percent versus 11.0 percent across our entire analysis sample), and are more likely to be located in the St. Louis area (43.4 percent versus 19.8 percent across our entire analysis sample), where the shooting occurred.

In addition to mechanically collecting more fines and fees, those municipalities in the top quartile of fines and fees also stop more vehicles. In particular, they stop roughly twice as many vehicles per capita (1.185 stops per capita versus 0.519 across our entire analysis sample).

There are similar patterns when considering our other two subsamples, highway traffic and SB5. Importantly, there is a large disparity in revenues collected between places above the 20% SB5 threshold and our analysis sample. In particular, municipalities that were above the SB5 threshold at baseline collected an average of $250 per person from fines and fees, as compared to only $69 per person across all municipalities in our analysis sample.

While there is clearly overlap in these subsamples (e.g., they are all more likely to be located in the St. Louis area), they are also not identical. Thus it is plausible that each of our subsamples captures distinct characteristics of interest with respect to the impact of Ferguson. For example, only 44 percent of municipalities in the top quartile of collections, and only 29 percent of municipalities with above median traffic, are expected to be impacted by SB5.

Overall, the fact that all three subsamples are associated with higher collections is consistent with a profit motive; yet, these differences in collections 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 difference-in-difference and event study approaches. Importantly, because these methods involve comparing trends over time, they eliminate the influence of time-invariant, baseline differences of the sort documented in panel A of table 1. We describe these empirical strategies in more detail in the ensuing sections.

5 Revenue from Fines and Fees in the Aftermath of Ferguson

We begin our analysis by documenting the evolution of collections from fines and fees in the aftermath of Ferguson. Thus, in this section, we will consider the Ferguson shooting as the treatment, which in turn triggered both the public outrage as well as the reforms implemented by the state of Missouri that were meant to curb policing for profit. For now, we remain agnostic about the relative importance of the policy reforms versus public scrutiny, an issue that we explore

in the final section.

Collections in the aftermath of Ferguson. 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 state fiscal year 2013. As noted above, the shooting occurred in August 2014, just two months into state fiscal year 2015. Following the shooting, there was a small drop in collections during state fiscal year 2015 and then larger drops in 2016 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, which ended June 30, 2014 and was thus the final fiscal year prior to the shooting, and fiscal year 2019, the end of our sample period.

We next investigate which municipalities were driving these post-Ferguson declines in collections. To the extent that these declines in Figure 1a are driven by changing incentives around police for profit, either because of post-Ferguson scrutiny or the reforms that passed in its aftermath, we expect the declines to be concentrated in municipalities engaged in policing for profit. Thus, we examine two subsets of municipalities. The first subset includes municipalities that raised a large amount of revenue-per-capita from fines and fees at baseline, specifically those in the "Q4" column of table 1, which we take as a measure of municipalities that were likely engaging in policing for profit. The second subset concerns municipalities that had large volumes of interstate highway traffic, specifically those described in the "traffic" column of 1, and hence had the motive and opportunity to police for profit via tax-exporting. We now consider each sub-sample in turn.

Figure 1b plots the raw time series of collections for each of the four quartiles. Notably, 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 to fund local services.

For the high-fine group, collections per-capita collapse in the aftermath of Ferguson. As evidenced by Figure 1b, the downward trend continues until fiscal year 2017, at which point collections per-capita level off at less than 100 dollars per capita. Thus, total collections in this group fall by more than 150 dollars per capita. While there are also post-Ferguson declines in the other quartiles, these are significantly smaller in both absolute per-capita and in percentage terms.

Motivated by this figure, we now formally test for a differential response to Ferguson in this subsample. Specifically, we estimate models of the form:

$$Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times \mathbb{1}_{m \in Q4} + \varepsilon_{mt}, \tag{1}$$

where Y_{mt} is collections per capita in municipality m in fiscal year t; α_m is a municipality fixed effect; $s = s(m)$ denotes whether municipality m was in the St. Louis metro area;[23] $\delta_{ts(m)}$ is a 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;[24] and $\mathbb{1}_{m \in Q4}$ is an indicator variable that takes a value of 1 for all municipalities who are in the fourth quartile based on the distribution of fines-per-capita in fiscal year 2012. For inference, we use standard errors that are calculated by clustering at the municipality level.

Figure 2a reveals a dramatic, and differential, decline in collections following the events of Ferguson. Initially, we see that collections per capita fell slightly in state fiscal year 2015, the year in which the shooting occurred. Thus, this coefficient should be viewed as "partially treated" from the perspective of the event, since the shooting occurred during the second month of the fiscal year. Subsequently, we see much more substantial declines during years 2016-2019, the period of time that municipalities are treated for the entire fiscal year. Notably, these declines are large in magnitude, leveling off around 80 dollars per capita by the end of the sample period. They are also statistically different from zero at conventional levels. Overall, we document substantial declines in collections per capita following the shooting in Ferguson, and it appears that these declines are driven by places that were raising large amounts of revenue at baseline, prior to the shooting.

Next we examine municipalities with the opportunity and motive to engage in policing for profit. As described in the preceding section, we develop a measure of average annual daily traffic (AADT) on highways in each municipality at baseline using the traffic data described in Appendix B.[25] 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. Thus we expect municipalities with large volumes of highway traffic have both the motive and opportunity to engage in tax exporting. Since our measure of highway traffic is continuous, we explore this question using a modified version of model 1 that replaces

[23] We define the St. Louis metro area be St. Louis City plus municipalities in St. Louis County. We include this interaction out of an abundance of caution with respect to potential differences between St. Louis and the rest of the state over this time period; however, the results are broadly similar if we do not include this interaction.

[24] More precisely, t^* represents fiscal year 2014 for collections. We retain the more general notation since it will be helpful when we adapt this model to outcomes measured in calendar years later on in this section

[25] 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.

the Q4 treatment indicator ($\mathbb{1}_{m \in Q4}$) with a continuous measure of baseline highway traffic.[26]

Figure 2b reveals that places with high levels of highway traffic see much larger declines in collections. Specifically, we observe that places with significant highway traffic, relative to places with less highway traffic, experience a small 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. Overall, there is a relative post-Ferguson decline in policing in places with significant traffic at baseline—a feature which provides both an opportunity and a motive to engage in policing for profit.

Robustness. As an alternative measure of the opportunity and motive for policing for profit, we also consider the role of interstate highway penetration. We begin by documenting a relationship between interstate penetration and collections from policing at baseline. As shown in Figure A.3a, 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.3b. 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 following the shooting in Ferguson. We define the interstate ratio as the interstate length in the municipality scaled by square miles. As shown in Figure A.4a, 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 larger declines in 2016 and 2017, with some reversion back towards zero in 2018 and 2019.

Police behavior in the aftermath of Ferguson. To summarize, our results thus far document a significant decrease in collections 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 significant highway traffic and interstate presence, both of which provide an opportunity and a motive to engage in policing for profit.

Yet, collections depend upon the actions of both the police and the court system. Thus, our

[26] Specifically, we estimate $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times X_m + \varepsilon_{mt}$ where X_m is our measure of baseline highway traffic and all other variables are as defined previously.

documented decline in revenue could be driven by either changes in the behavior of the police in response to Ferguson or changes in enforcement via the courts. The frequency with which police officers stop vehicles provides a measure of police behavior that is not directly dependent on the courts. This motivates our analysis of police stops as an outcome.

We begin our analysis of stops with simple time series evidence. As shown in Figure A.1a, police stops decline immediately in calendar year 2015, which began just after the shooting in August 2014. In particular, stops averaged just over 0.50 stops per capita, prior to the shooting in Ferguson but declined to roughly 0.40 stops per capita following the shooting. Thus, on average across municipalities, stops declined by roughly 20 percent.

We next consider the source of this drop in stops. To do so, we estimate model 1 using stops as the outcome.[27] As shown in Figure A.1b, the decline in stops was again driven by places that raised significant amounts from policing at baseline. As shown in Figure A.2a, these declines in Q4, relative to the declines in Q1-Q3, average about 0.2 stops per capita and these post-Ferguson differences are statistically significant at conventional levels. Likewise, as shown in Figure A.2b the decline in stops was driven by places with the motive and opportunity to engage in policing for profit, as measured by baseline highway traffic levels in the municipality.[28]

One possible explanation for the decline in stops could be that police withdrew effort in the aftermath of Ferguson—popularly termed the "Ferguson Effect." If it were the case that police simply stopped fewer vehicles in response to the outrage, we would expect the decline in stops to be uniformly distributed across different types of stops or otherwise concentrated in marginal stops that would have resulted in a warning. Thus, we next compare stops that resulted in citations, which generate revenue, and warnings, which do not generate revenue.

Figure 3 reveals that the differential decline in stops among municipalities likely engaged in policing for profit is concentrated in stops that produce revenue. Specifically, in Figure 3a, the decline in citations mirrors the pattern in the overall reduction in stops, with a post-Ferguson decline in citations in Q4 relative to Q1-Q3 equal to roughly 0.20. Results examining warnings, as shown in Figure 3b, reveal no evidence of a post-Ferguson decline in warnings issued by police. Moreover, Figures 3c and 3d reveal that the results are similar when examining highway traffic as a measure of the opportunity and motive to engage in policing for profit.[29] Taken together, this evidence is again consistent with a post-Ferguson decline in policing for profit in places raising

[27] We also adjust the treatment year t^* appropriately to reflect that fact that the stops data are recorded in calendar years, rather than fiscal years.

[28] The results for our alternative measure, interstate penetration, are shown in Figure A.4b. These results are also document that the decline in stops is driven by places with the motive and opportunity to engage in policing for profit.

[29] Results using our alternative measure, as shown in Figure A.5, are more noisy, with some evidence of declines in both citations and warnings in places with interstate penetration.

significant amounts at baseline.

To summarize, we find significant declines in both collections and stops following the shooting in Ferguson. This is driven by places raising significant amounts at baseline and in places with the motive and opportunity to engage in policing for profit at baseline. As further evidence that the decline in policing was driven by revenue incentives, we find that the decline in stops is concentrated in stops that result in citations, which raise revenue, relative to stops that result in warnings, which do not raise revenue.

6 Downstream Outcomes: Safety and Racial Equity

Our results to this point raise two important questions related to downstream outcomes. The first concerns safety. In theory, policing for profit could be justified if the incentives created by revenue sharing of fines and fees leads to increased effort on the part of law enforcement and an overall improvement in public safety.[30] Therefore, it is important to ask whether any post-Ferguson shifts in policing caused downstream changes in the likelihood of safety outcomes that are potentially related to traffic tickets (e.g. accidents).

The second concerns racial equity. Much of the outrage following Ferguson concerned the racial implications of policing for profit. If policing for profit does disproportionately impact 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 racial groups in the frequency and nature of their contact with the criminal justice system. Therefore, it is important to ask whether the documented reduction in policing for profit improved racial equity in Missouri. In this section, we explore both of these important questions in turn.

6.1 Policing for Profit and Safety

To explore safety, we leverage incident level data covering the universe of traffic accidents in Missouri. Importantly, the incident-level data allow us to explore the implications of policing for profit 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

[30] 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.

(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 an interstate 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. Figure 4a plots the average number of monthly traffic accidents per-capita for municipalities in Q4 in terms of baseline collections per capita relative to municipalities in Q1-Q3. Overall, there does appear to be spike in accidents following the shooting in Ferguson. Moreover, the gap between Q4 and Q1-Q3 seems to grow post-Ferguson, relative to pre-Ferguson, suggesting that the increase in accidents was larger in places that were engaging in policing for profit at baseline.

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

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

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_t is a post-Ferguson indicator and $1_{m \in M^*} = 1(m \in M^*)$ is an indicator that takes a value of 1 if the municipality was engaged in policing for profit at baseline.

The key parameter from this regression model is β, which measures the change in trend following the shooting in Ferguson in places that engaged in policing for profit at baseline. 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.

Figure 4b gives formal event study estimates of the variation on display in the raw trends. In this figure, the x-axis denotes time relative to the Ferguson shooting. 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 shooting in Ferguson. As shown, we again find some evidence of an increase in the number of accidents following the

shooting in Ferguson in places that were at in the upper quartile of collections per capita at baseline.

To improve power, Table 2 also reports 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 again find some evidence of changes in safety. In particular, the number of vehicles involved in accidents each month rises by 0.318, an increase of 30 percent relative to the control mean, and is statistically significant at conventional levels. This pattern is echoed by results for the overall number of vehicles (0.637, relative to a control mean of 1.954), the number of injuries (0.159, relative to the control mean of 0.339), and the number of minor injuries (0.153, relative to the control mean of 0.328). We find no evidence of a statistically significant effect for the number of disabling injuries. We next focus on accidents that occur on interstate highways, the idea being that police engaged in policing for profit might particularly target non-residents on these highways. As shown in panel B, the results are similar when focusing on accidents that occur on these interstate highways.

We next focus on places with the motive and opportunity to engage in policing for profit. As shown in Table 3, we again find post-Ferguson increases in accidents in places with significant highway traffic at baseline, and this is also true when focusing on accidents that occur on interstates, as shown in panel B.[31]

Thus, taken together, we argue that evidence in this section is consistent with the idea that the post-Ferguson reduction in policing and revenue might have negatively impacted public safety. Having established this, we now turn to the implications of the retained revenue cap for racial equity.

6.2 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, a report on policing for profit in the aftermath of the Ferguson shooting 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, prior to

[31] Likewise, as shown in Table A.1, we find increases in the number of accidents in places with significant interstate penetration.

the shooting in Ferguson.[32] Given these patterns and motivations, reforms around policing for profit might disproportionately benefit black drivers in Missouri via an improvement in racial disparities.

While our collections data do not have any information on race, we can use the stops data, which do include information on race, to decompose the post-Ferguson reduction in stops into white stops and black stops. More specifically, we decompose the overall reduction in stops per capita into stops of black drivers per capita and white drivers per capita. As shown in Figure 5a, the overall post-Ferguson decline in stops per capita in Q4 of the collections distribution, relative to Q1-Q3, is driven by stops of white drivers. For black drivers, by contrast, Figure 5b shows the results are small in magnitude, close to zero, and statistically insignificant. This difference is also documented in Table 4, which pools the years into pre-Ferguson and post-Ferguson periods.

Likewise, when examining the motive and opportunity to engage in policing for profit, proxied by highway traffic, the results are again driven by stops of white drivers, relative to stops of black drivers (see also Figures 5c and 5d, and Table 5).[33]

To summarize, the reduction in stops following the shooting in Ferguson in places engaged in policing for profit is explained by stops of white drivers rather than black drivers. This finding 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 Ferguson. Black drivers, by contrast, experienced no reduction in stops. This is despite the fact that opponents of policing for profit 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, the post-Ferguson decline in stops of white drivers arguably exacerbated pre-existing inequalities in policing by race.

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

[33] As shown in Figure A.7 and Table A.2, results are similar when using interstate penetration, our alternative measure of the motive and opportunity to engage in policing for profit.

7 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. Given the structure of the cap, SB5 also provides a clear shift in revenue incentives, allowing us to shed light on the objective function of the criminal justice system in Missouri. 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.

7.1 Research Design

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

$$Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times 1_{m \in M^*} + \varepsilon_{mt} \tag{3}$$

where Y_{mt} is an outcome variable (typically court collections per capita) 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 (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 (3) 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 in treated municipalities ($m \in M^*$) would have experienced had 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 differentially 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 7.3.

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 reached. 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.

7.2 Event-study Estimates

Figure 6 contains the key results from the event study model. As shown, 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 7 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 7 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 6 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.

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 6, 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. As shown in column 1 of Panel A, we find that collections per capita decline by $107.1 (row 1) 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.

7.3 SB5 versus post-Ferguson scrutiny

Our evidence on SB5, which documents a differential post-Ferguson change for municipalities treated by SB5, is consistent with a change in the incentives for revenue-driven policing. However, it is possible that SB5 treatment may be correlated with other municipality characteristics that would also predict a decline in the presence of increased scrutiny following the shooting, such as highway traffic or 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.[34] As shown in column 2 of Table 6 (Panel A), the estimated effects in St. Louis are larger than our main estimates in column 1. 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 associated with SB5. 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

[34] 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.

Ferguson. As shown in column 4 of Table 6, 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. 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 post-Ferguson scrutiny. These results are presented in Panel B of Table 6. 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.[35] 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. 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 coefficient.

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 6 above, collections fell only slightly in 2015, the year following the Ferguson shooting, but the decline accelerated in 2016, the year in which SB5

[35]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.

was introduced. This idea is also captured in Figure 7, 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 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.[36] 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 8, 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 100-150 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 piece of evidence, we examine the salience of the 20 percent cap on retained revenue. If the post-SB5 declines in policing are driven by other factors, the fact that the cap binds exactly at 20 percent should not matter. By contrast, if SB5 is driving our results and if municipalities are maximizing revenue, then municipalities should lower their collections below 20 percent. We first investigate this issue by measuring the fraction of general revenues from collections. While we already have a baseline measure, based upon general revenue over the period 2011-2013, we also create a post-SB5 measure based upon general revenue over the period 2016-2018.[37] As shown in Figure 9, control places, which, by definition, raise less than 20 percent of revenue from policing at baseline, also tend to raise less than 20 percent during the post-SB5 period, with a very small number of municipalities raising more than 20 percent. There are bigger changes in the treatment group, which, by definition, raise more than 20 percent of revenue from policing at baseline. As shown, roughly 80 percent of treated municipalities

[36] 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.

[37] Similarly, to our baseline measure, we average general revenue from the CAFRs over this period 2016-2018. For places with no CAFRs reports over these three years, we use general revenue from the 2017 Census of Governments.

raise less than 20 percent at baseline. To summarize, we find that the vast majority of treated municipalities that raised more than 20 percent at baseline raise less than 20 percent post-SB5. This is difficult to reconcile with other factors that might have caused declines in collections in these places.

Building upon this idea, we next run a regression that incorporates a continuous version of our treatment. In particular, our treatment is now the minimum of zero and the difference between the share from policing at baseline and 20 percent. Thus, for places raising more than 20 percent at baseline, the continuous treatment captures how far they are above 20 percent. For places raising less than 20 percent at baseline, the continuous treatment is equal to zero. Our dependent variable is then the share of revenue from policing. In the context of this regression, if all municipalities raising above 20 percent at baseline go down to the cap of 20 percent, then the coefficient on the continuous treatment interacted with the post-SB5 indicator should equal negative one. As shown in Table 7, we find evidence consistent with this basic idea. In particular, the coefficient ranges from -0.956 in column 1 to -0.915 in column 3. Moreover, all confidence intervals include negative one, meaning that we cannot reject the null hypothesis of full adjustments down to the cap of 20 percent. This evidence is again consistent with the cap of 20 percent being particularly salient in driving the post-SB5 reductions in collections.

7.4 Policing Resources

We next attempt to answer the following question: Is the post-SB5 reduction in collections 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 driven, at least in part, by a reduction in resources? Police departments treated by SB5 could experience a reduction in resources for at least two reasons. First, municipalities with pure revenue motives around policing might face incentives to scale back resources in the presence of a cap on retained revenues. And, second, municipalities might have experienced a negative fiscal shock associated with SB5, leading to an overall reduction in government spending and payroll.

To answer this question, we incorporate data on the size of the police force and policing budgets using data from the Census of State and Local Law Enforcement. Overall, as detailed in Online Appendix Section 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 documented in this paper appears to be driven by a reduction in policing effort holding resources fixed.

8 Conclusion

Our results document a clear and consistent post-Ferguson decline in policing for profit. 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 explore two implications of the documented reduction in policing for profit. First, we find evidence of a corresponding reduction in safety, suggesting a tradeoff between revenue-driven policing and safety. Second, despite the fact that the post-Ferguson spotlight on policing for profit was motivated by racial concerns around over-policing of black drivers, we find that the reform led to a reduction of stops for white drivers but not for black drivers. We then document treatment effects of SB5, which limited retained revenues from policing to 20 percent of general revenue, on policing for profit. We find significant reductions in collections, and we argue our results cannot be explained by any post-Ferguson increase in scrutiny of police. 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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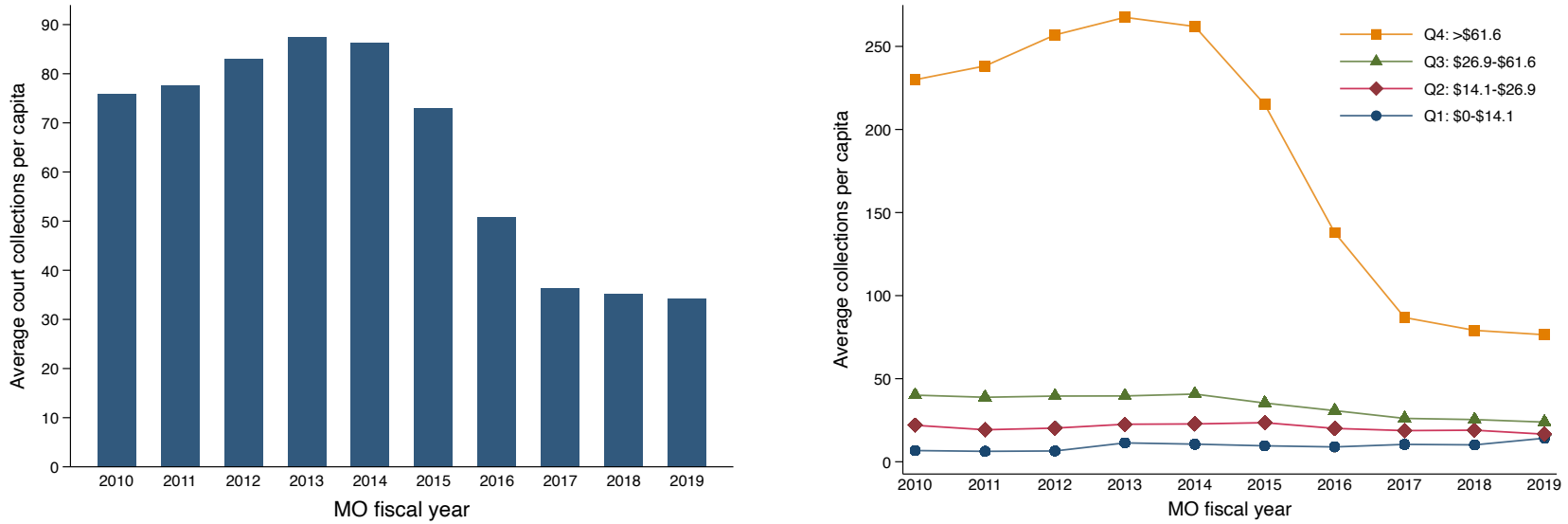
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Table 1: Summary Statistics

	Panel A: Demographics and Geography				
	All	Analysis	Q4	Traffic	SB5
Population (2010)	7,922	9,498	4,284	3,211	1,723
White share	84.8%	83.0%	69.8%	77.7%	68.6%
Black share	9.8%	11.0%	23.8%	16.8%	26.0%
Other race share	5.5%	6.0%	6.5%	5.5%	5.4%
Interstate ratio	0.158	0.178	0.303	0.307	0.288
Any Interstate	29.4%	33.3%	43.4%	44.5%	34.5%
AADT percentile	44.5	44.4	67.2	72.9	75.7
St. Louis County/City	17.7%	19.8%	43.4%	35.4%	41.8%
	Panel B: Fines and Fees				
	All	Analysis	Q4	Traffic	SB5
Exceed SB5 20% Threshold	14.4%	14.3%	44.3%	29.3%	100.0%
Total collections per capita	83.027	68.760	183.717	118.351	249.979
Collections per capita quartile = 4	25.1%	27.6%	100.0%	47.6%	85.5%
	Panel C: Policing Outcomes				
	All	Analysis	Q4	Traffic	SB5
Stops per capita	0.587	0.519	1.185	0.834	1.648
Stops per capita (White)	0.484	0.431	0.936	0.671	1.318
Stops per capita (Black)	0.081	0.070	0.205	0.136	0.277
Citations per capita	0.355	0.307	0.790	0.524	1.143
Warnings per capita	0.236	0.216	0.407	0.313	0.543
Municipalities	470	384	106	164	55

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 column labeled "Analysis" is restricted to the subsample of municipalities in the "All" column that are approximately balanced, year-over-year, in terms of the existence of collections, specifically, those municipalities where we can observe collections for at least 4 years pre- and post-Ferguson. The "Q4" column contains analysis municipalities in the top quartile of average collections per capita over 2010–2012. The "Traffic" column contains analysis municipalities above the median of annual average daily traffic (AADT) per capita over 2011–2013. The "SB5" column contains analysis municipalities that were above the SB5 20% threshold over 2011–2013. 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 2010 Census (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, the percentile of AADT per capita over 2011–2013, whether the municipality is located inside of St. Louis city or St. Louis county). Panel B contains variables constructed from the court data: total collections per capita, an indicator for whether the municipality was above the SB5 20% threshold over 2011–2013, and an indicator for being in the fourth (highest) quartile of the distribution of fines per capita over 2010–2012. 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.

Figure 1: Collections declined after Ferguson and were concentrated in "high-fine" municipalities

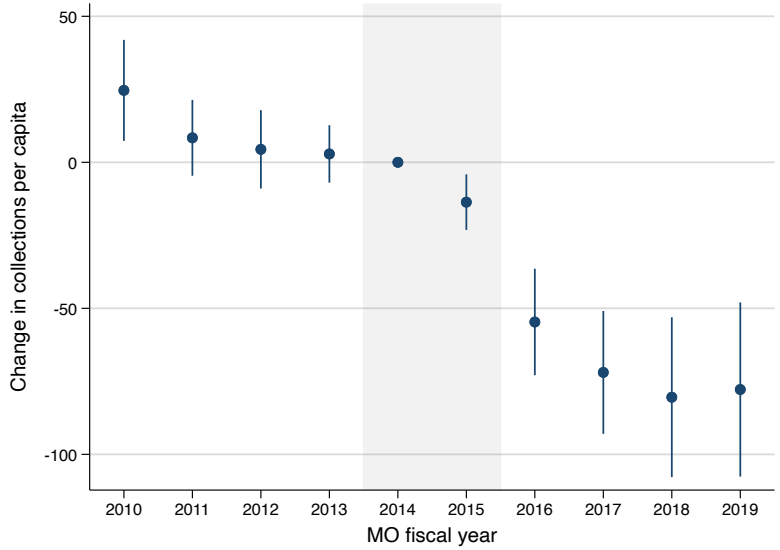


(a) Collections per capita by year

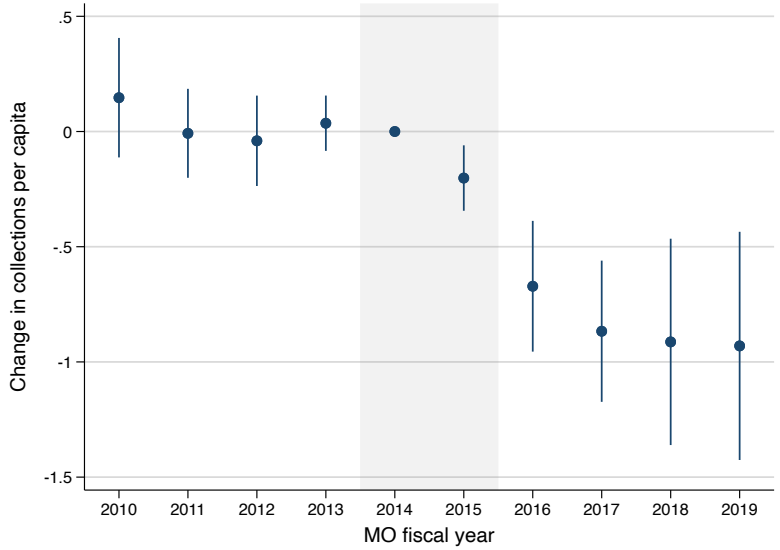
(b) Collections per capita by quartiles

Notes: In this exhibit, we plot statewide trends for the average municipality in Missouri for court collections per capita and provide evidence that the declines occurred almost exclusively in municipalities that were raising excess revenue at baseline. Note that the x-axis is represented in "fiscal years" to better align with the court collections data. The time series (Panel A) exhibit significant declines (57% for collections) after the killing of Michael Brown in 2014 (fiscal year 2015). We then divide our sample of municipalities into four quartiles based on the distribution of collections per capita across fiscal years 2011–2013. The time series by quartile (Panel B) shows that the lowest three quartiles (Q1-Q3 in the figures) see little evidence of declines in court collections, but the highest quartile (Q4) sees large declines in the aftermath of the killing of Michael Brown in 2014 (fiscal year 2015).

Figure 2: High-collections and high-traffic municipalities also see relative declines in collections



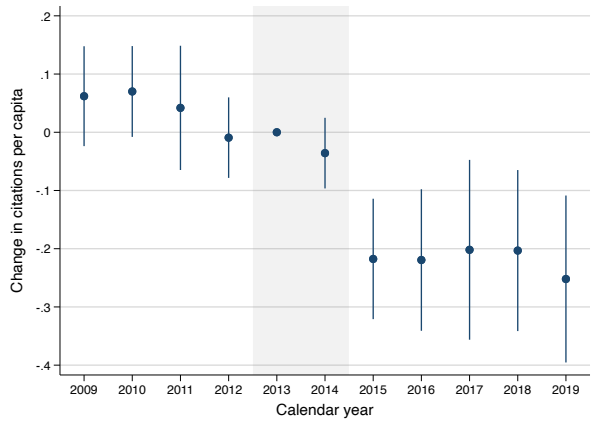
(a) Collections per capita (Q4 vs. Q1-Q3)



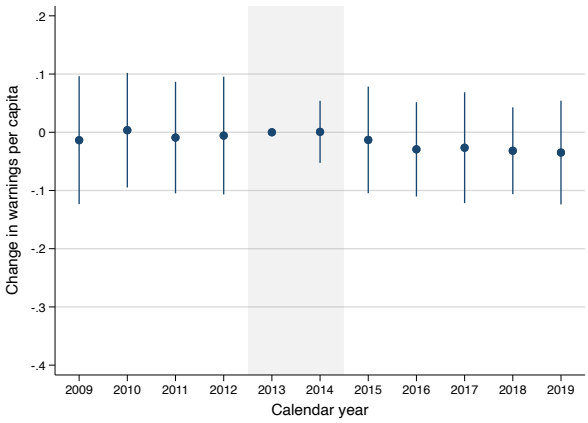
(b) Collections per capita (AADT)

Notes: This exhibit provides further evidence that the statewide decline in court collections occurred almost exclusively in municipalities that were best positioned to raise excessive revenue at baseline. Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed collections (e.g. the sample in the "Analysis" column of table 1) to estimate models of the form: $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times T_m + \varepsilon_{mt}$ where Y_{mt} is court collections per capita in municipality m in 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 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^* = fiscal year 2014 for collections); and T_m is the treatment measure. The killing took place two months into the 2015 fiscal year, making 2015 partially treated. We lightly shade fiscal years 2014 and 2015 in gray to indicate the years right before full treatment. In Panel A, $T_m = \mathbb{1}_{m \in Q4}$, an indicator variable that takes a value of 1 for all municipalities who are in the fourth quartile based on the distribution of collections per capita over 2010–2012. In Panel B, T_m is the municipality's percentile in the distribution of average annual daily traffic (AADT) per capita over 2011–2013. Note that the x-axis is represented in "fiscal years" to better align with the court collections 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 in municipalities that were raising excessive revenue at baseline (Panel A) and with higher traffic volumes (Panel B).

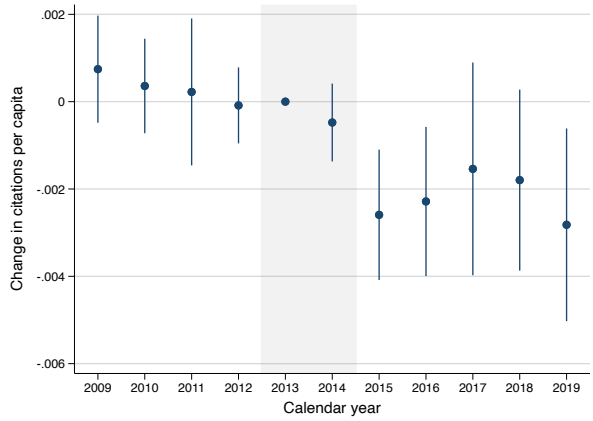
Figure 3: Decline in stops was concentrated in stops that raise revenue



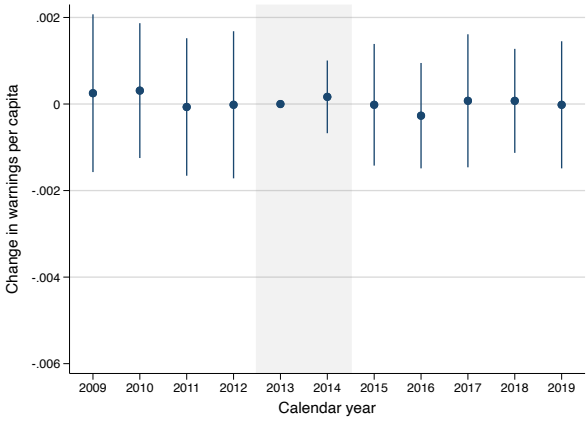
(a) Citations per capita (Q4 vs. Q1-Q3)



(b) Warnings per capita (Q4 vs. Q1-Q3)



(c) Citations per capita (AADT)

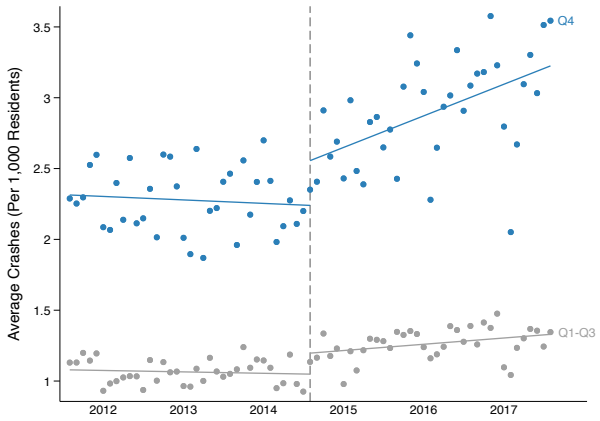


(d) Warnings per capita (AADT)

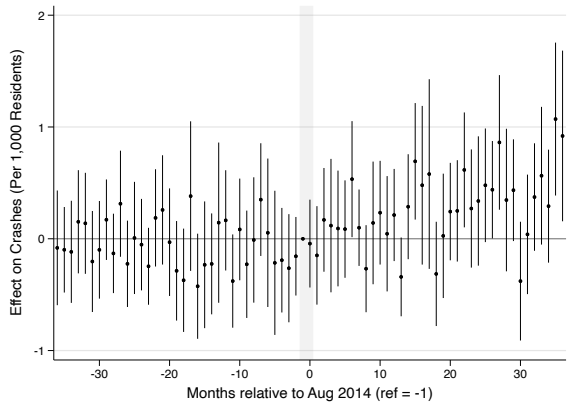
Notes: This exhibit provides evidence that the statewide decline in traffic stops among municipalities that were likely engaged in policing for profit is concentrated in stops that raise revenue. Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed stops to estimate models of the form: $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times T_m + \varepsilon_{mt}$ where Y_{mt} is citations per capita (Panels A and B) or warnings per capita (Panels C and D) in municipality m in calendar 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 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); and T_m is the treatment measure. In Panels A and C, $T_m = \mathbb{1}_{m \in Q4}$, an indicator variable that takes a value of 1 for all municipalities who are in the fourth quartile based on the distribution of collections per capita over 2010–2012. In Panel B and D, T_m is the municipality's percentile in the distribution of average annual daily traffic (AADT) per capita over 2011–2013. Note that the x-axis is represented in calendar years to better align with the traffic stops data. The killing took place in August 2014, making calendar year 2014 partially treated. We lightly shade calendar years 2013 and 2014 in gray to indicate the years right before full treatment. 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 that the declines in stops are concentrated in citations (Panel A and B) with little corresponding change in warnings (Panel C and D).

Figure 4: Baseline collections and safety – visual evidence

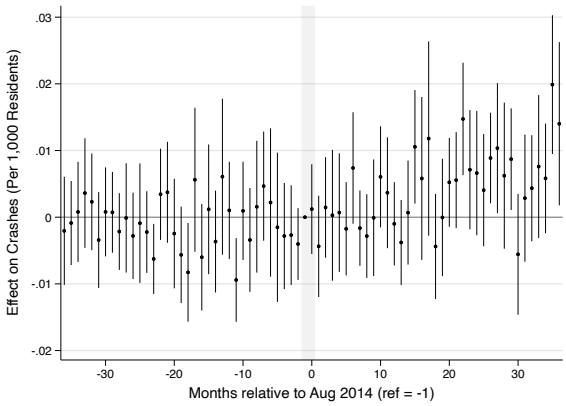
(a) Raw crash trends



(b) Event study (Q4 vs. Q1-Q3)



(c) Event study (AADT)



Notes: This figure presents evidence of the impact of Ferguson on safety related outcomes. Panel A displays raw crash trends across Missouri. Each dot represents crashes per 1,000 residents, measured monthly, and averaged across municipalities. Gray dots represent Q1–Q3 municipalities and blue dots represent Q4 municipalities, where quartiles are based on collections per capita over 2010–2012. The dashed vertical gray line denotes the month of Michael Brown's killing. Trend lines are fit to the binned averages separately for Q1–Q3 and Q4 municipalities within each segment. Panels (b) and (c) present event study coefficients from $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times T_m + \varepsilon_{mt}$ 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 municipality m was in the St. Louis metro area; $\delta_{ts(m)}$ is a date by St. Louis county fixed effect; $D_\tau = 1(t - t^* = \tau)$ is an event study indicator that takes a value of one exactly τ months relative to the killing of Michael Brown; and T_m is the treatment measure. In Panel B, $T_m = \mathbb{1}_{m \in Q4}$, an indicator variable that takes a value of 1 for all municipalities who are in the fourth quartile based on the distribution of collections per capita over 2010–2012. In Panel C, T_m is the municipality's percentile in the distribution of average annual daily traffic (AADT) per capita over 2011–2013. The killing took place in August 2014, thus we lightly shade July and August 2014 in gray to indicate the months right before full treatment. Standard errors are clustered at the municipality level. Vertical lines denote 95% confidence intervals.

Table 2: Baseline collections and safety

	Panel A: All Roads (Outcome per 1,000 Residents)				
	(1)	(2)	(3)	(4)	(5)
Q4 X Post Ferguson	0.318***	0.637***	0.159***	0.153***	0.004
	(0.095)	(0.182)	(0.052)	(0.055)	(0.015)
Outcome	Crashes	Vehicles	Injuries	Minor Inj.	Disabling Inj.
Observations	27229	27229	27229	27229	27229
Control mean	1.064	1.954	0.339	0.306	0.034
Municipalities	373	373	373	373	373
Adjusted R2	0.69	0.70	0.23	0.22	0.05
	Panel B: Interstates Only (Outcome per 1,000 Residents)				
	(1)	(2)	(3)	(4)	(5)
Q4 X Post Ferguson	0.136***	0.276***	0.055*	0.057*	-0.004
	(0.048)	(0.104)	(0.031)	(0.033)	(0.003)
Outcome	Crashes	Vehicles	Injuries	Minor Inj.	Disabling Inj.
Observations	27229	27229	27229	27229	27229
Control mean	0.096	0.165	0.026	0.023	0.003
Municipalities	373	373	373	373	373
Adjusted R2	0.64	0.67	0.19	0.18	0.03

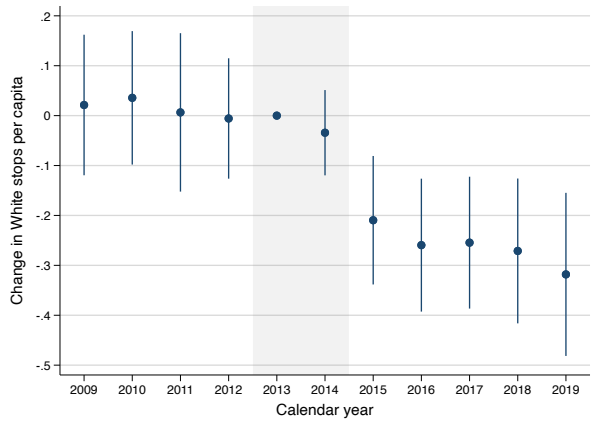
Notes: This table presents estimates of the impact of Ferguson on safety related outcomes. Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed stops, restricted to the 36 months before or after the killing of Michael Brown. Each number is generated from a regression of the form $Y_{mt} = \alpha_m + \delta_{ts(m)} + \beta D_t \times 1_{m \in Q_4} + \varepsilon_{mt}$ 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; $D_t = 1(t - t^* > 0)$ is an an indicator that takes a value of 1 for every month after the killing of Michael Brown (t^*) in municipality m; and $1_{m \in Q_4}$ is an indicator that takes a value of 1 if municipality m has baseline collections per capita over 2010–2012 in Q4. 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 4c. In Figure 4a we also display raw trends for all crashes in the St. Louis metro area, for Q4 vs Q1-3 baseline collections per capita. The control mean is the average outcome among Q1–Q3 municipalities before the killing of Michael Brown. Standard errors are clustered at the municipality level and displayed in parentheses, with stars denoting statistical significance at the 1 (***), 5(**), and 10% (*) levels.

Table 3: Baseline traffic and safety

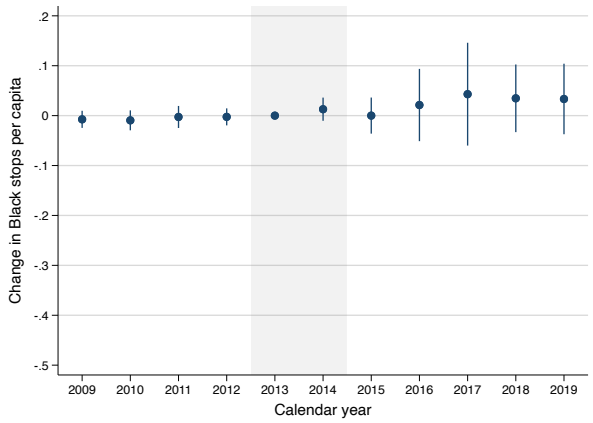
	Panel A: All Roads (Outcome per 1,000 Residents)				
	(1)	(2)	(3)	(4)	(5)
AADT X Post Ferguson	0.005***	0.010***	0.003***	0.003***	0.000
	(0.001)	(0.003)	(0.001)	(0.001)	(0.000)
Outcome	Crashes	Vehicles	Injuries	Minor Inj.	Disabling Inj.
Observations	27229	27229	27229	27229	27229
Pre-period mean	1.364	2.494	0.441	0.397	0.044
Municipalities	373	373	373	373	373
Adjusted R2	0.69	0.69	0.23	0.22	0.05
	Panel B: Interstates Only (Outcome per 1,000 Residents)				
	(1)	(2)	(3)	(4)	(5)
AADT X Post Ferguson	0.003***	0.005***	0.001**	0.001**	-0.000
	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)
Outcome	Crashes	Vehicles	Injuries	Minor Inj.	Disabling Inj.
Observations	27229	27229	27229	27229	27229
Pre-period mean	0.192	0.338	0.057	0.051	0.005
Municipalities	373	373	373	373	373
Adjusted R2	0.64	0.67	0.19	0.18	0.03

Notes: This table presents estimates of the impact of Ferguson on safety related outcomes. Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed stops, restricted to the 36 months before or after the killing of Michael Brown. Each number is generated from a regression of the form $Y_{mt} = \alpha_m + \delta_{ts(m)} + \beta D_t \times A_m + \varepsilon_{mt}$ 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; $D_t = 1(t - t^* > 0)$ is an an indicator that takes a value of 1 for every month after the killing of Michael Brown (t^*) in municipality m; and A_m is the municipality's percentile in the distribution of average annual daily traffic per capita over 2011–2013. 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 4c. The pre-period mean is the average outcome among all municipalities before the killing of Michael Brown. Standard errors are clustered at the municipality level and displayed in parentheses, with stars denoting statistical significance at the 1 (***), 5(**), and 10% (*) levels.

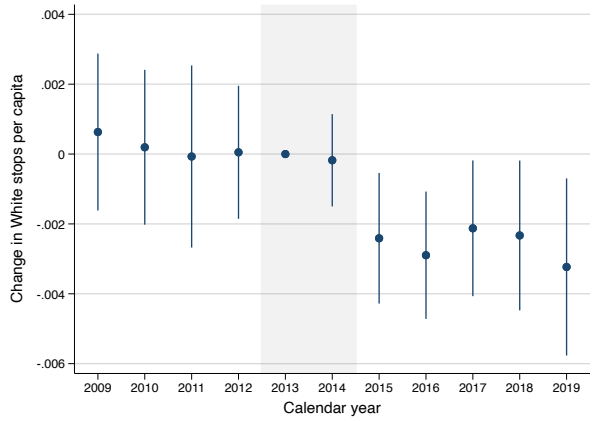
Figure 5: Decline in stops was concentrated in stops of White drivers



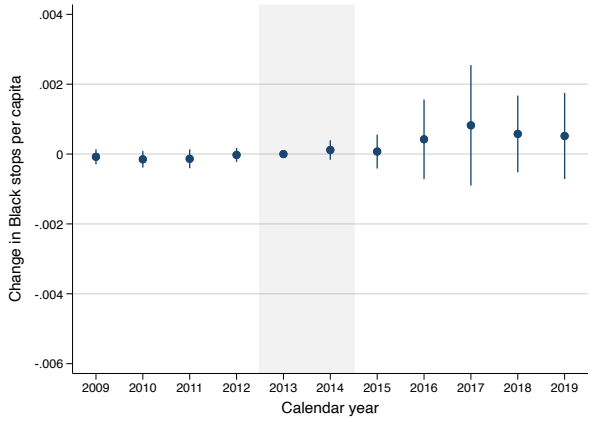
(a) White stops per capita (Q4 vs. Q1-Q3)



(b) Black stops per capita (Q4 vs. Q1-Q3)



(c) White stops per capita (AADT)



(d) Black stops per capita (AADT)

Notes: This exhibit shows that the post-Ferguson decline in stops in municipalities likely engaged in policing for profit stems from a decline in stops of White drivers, with no decline for Black drivers. Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed stops to estimate models of the form: $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times T_m + \varepsilon_{mt}$ where Y_{mt} is stops of White drivers (Panels A and B) or stops of Black drivers (Panels C and D) in municipality m in calendar 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 year by St. Louis area fixed effect; $D_\tau = 1(t - t^* = \tau)$ is an indicator that takes a value of one τ years relative to the killing of Michael Brown (where t^* = calendar year 2013); and T_m is the treatment measure. In Panels A and C, $T_m = \mathbb{1}_{m \in Q4}$, an indicator variable that takes a value of 1 for all municipalities who are in the fourth quartile based on the distribution of collections per capita over 2010–2012. In Panel B and D, T_m is the municipality's percentile in the distribution of average annual daily traffic (AADT) per capita over 2011–2013. Note that the x-axis is represented in calendar years to better align with the traffic stops data. The killing took place in August 2014, making calendar year 2014 partially treated. We lightly shade calendar years 2013 and 2014 in gray to indicate the years right before full treatment. 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 a large relative decline in stops for White drivers (Panel A and B) but no corresponding decline for Black drivers (Panel C and D).

Table 4: Baseline collections and stop characteristics

	(1) Cites pc	(2) Warnings pc	(3) B stops pc	(4) W stops pc
Q4 X Post Ferguson	-0.221***	-0.017	0.029	-0.235***
	(0.052)	(0.036)	(0.029)	(0.048)
Observations	4034	4034	4034	4034
Control mean	0.123	0.145	0.018	0.242
Municipalities	373	373	373	373
Adjusted R2	0.80	0.78	0.58	0.85

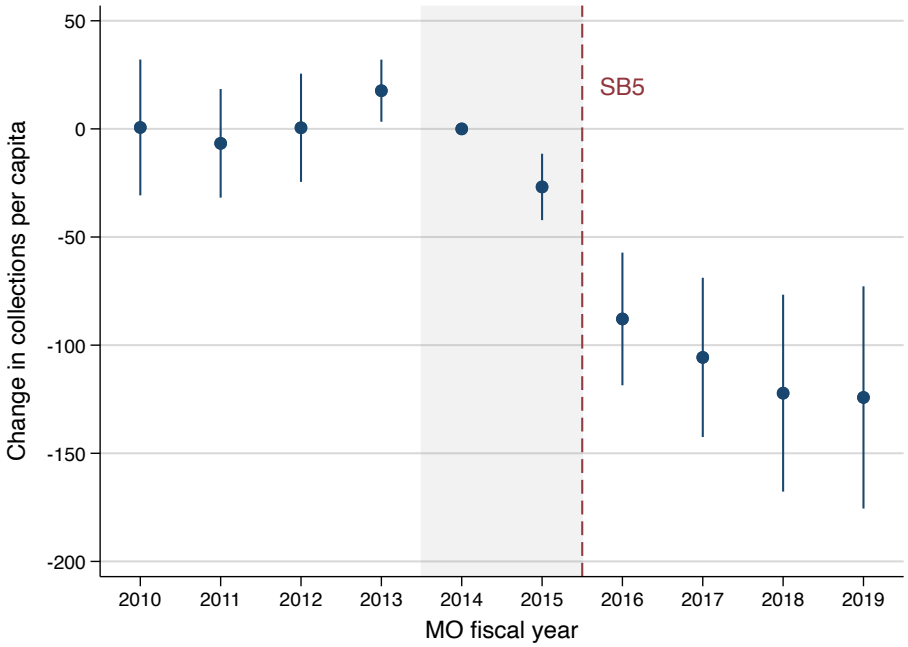
Notes: This table shows that the decline in stops among municipalities with high baseline collections per capita—a potential indicator of policing for profit—was concentrated in stops that raise revenue and stops of White drivers. Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed stops. Each coefficient is generated from a regression of the form $Y_{mt} = \alpha_m + \delta_{ts(m)} + \beta D_t \times 1_{m \in Q_4} + \varepsilon_{mt}$ where Y_{mt} is either a stop outcome (citation or warning), or the count of stops by race of the driver (Black, White). The term 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; $D_t = 1(t - t^* > 0)$ is an an indicator that takes a value of 1 for every month after the killing of Michael Brown (t^*) in municipality m; and $1_{m \in Q_4}$ is an indicator that takes a value of 1 if municipality m has baseline collections per capita over 2010–2012 in Q4. The dependent variables in columns 1 and 2 are citations and warnings per capita, and correspond to the event studies shown in Figures 3a and 3b. The dependent variables in columns 3 and 4 are stops of Black and White drivers per capita, and correspond to the event studies in Figures 5b and 5a. The control mean is the average outcome among Q1–Q3 municipalities before the killing of Michael Brown. Standard errors are clustered at the municipality level and displayed in parentheses, with stars denoting statistical significance at the 1 (***), 5 (**), and 10% (*) levels.

Table 5: Baseline traffic and stop characteristics

	(1) Cites pc	(2) Warnings pc	(3) B stops pc	(4) W stops pc
AADT X Post Ferguson	-0.002**	-0.000	0.000	-0.002***
	(0.001)	(0.001)	(0.000)	(0.001)
Observations	4030	4030	4030	4030
Pre-period mean	0.281	0.208	0.054	0.411
Municipalities	373	373	373	373
Adjusted R2	0.79	0.78	0.58	0.85

Notes: This table shows that the decline in stops among municipalities with high baseline traffic—a measure of policing for profit opportunity—was concentrated in stops that raise revenue and stops of White drivers. Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed stops. Each coefficient is generated from a regression of the form $Y_{mt} = \alpha_m + \delta_{ts(m)} + \beta D_t \times A_m + \varepsilon_{mt}$ where Y_{mt} is either a stop outcome (citation or warning), or the count of stops by race of the driver (Black, White). The term 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; $D_t = 1(t - t^* > 0)$ is an an indicator that takes a value of 1 for every month after the killing of Michael Brown (t^*) in municipality m; and A_m is the municipality's percentile in the distribution of average annual daily traffic per capita over 2011–2013. The dependent variables in columns 1 and 2 are citations and warnings per capita, and correspond to the event studies shown in Figures 3c and 3d. The dependent variables in columns 3 and 4 are stops of Black and White drivers per capita, and correspond to the event studies in Figures 5d and 5c. The pre-period mean is the average outcome among all municipalities before the killing of Michael Brown. Standard errors are clustered at the municipality level and displayed in parentheses, with stars denoting statistical significance at the 1 (***), 5 (**), and 10% (*) levels.

Figure 6: Reductions in court collections from SB5 (20% threshold on retained revenue)



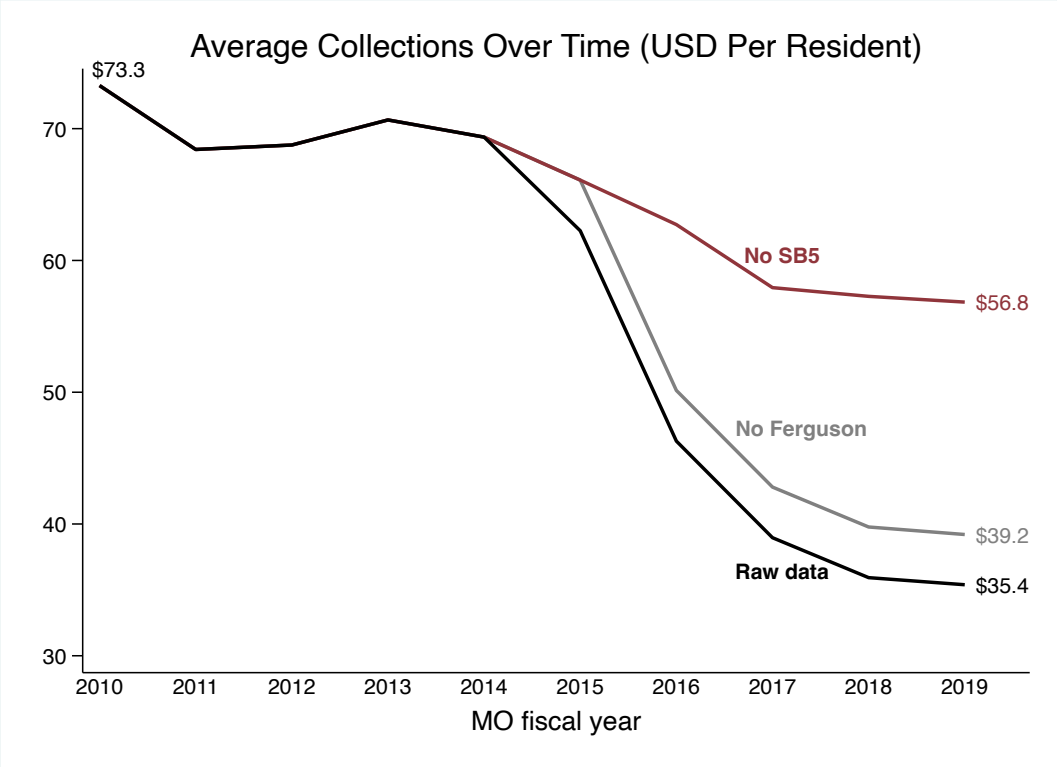
Notes: In this exhibit, we provide evidence that the SB5 20% threshold on retained revenue reduced court collections. Specifically, we use the sample of municipalities contained in the "Analysis" columns of table 1 to estimate models of the form: $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times \mathbb{1}_{m \in M^*} + \varepsilon_{mt}$ where Y_{mt} is court collections per capita in municipality m in fiscal 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 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^* = fiscal year 2014 for collections); M^* denotes the set of municipalities that were above the SB5 20% cap on retained revenue at baseline (on average over 2011–2013); and $\mathbb{1}_{m \in M^*}$ 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. Note that the x-axis is represented in "fiscal years" to better align with the court collections data. The killing took place two months into the 2015 fiscal year, making 2015 partially treated. We lightly shade fiscal years 2014 and 2015 in gray to indicate the years right before full treatment. The y-axis 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 in municipalities that were subject to the SB5 20% limit on retained revenue.

Table 6: Examining the 20% cap on municipal revenue from fines and fees

	Panel A: Alternative Samples				
	(1)	(2)	(3)	(4)	(5)
Collections	-107.137***	-147.355***	-84.084***	-134.602***	-105.278***
	(19.161)	(33.612)	(22.747)	(42.120)	(22.057)
Sample	All	Inside STL	Outside STL	Interstate	4th Quartile
Observations	3805	759	3046	1276	1053
Control mean	39.097	68.720	33.398	49.502	104.501
Municipalities	384	76	308	128	106
Adjusted R2	0.85	0.78	0.88	0.87	0.82
	Panel B: Alternative Specifications				
	(1)	(2)	(3)	(4)	
Collections	-107.137***	-103.741***	-78.949***	-102.600***	
	(19.161)	(19.251)	(15.525)	(18.721)	
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	

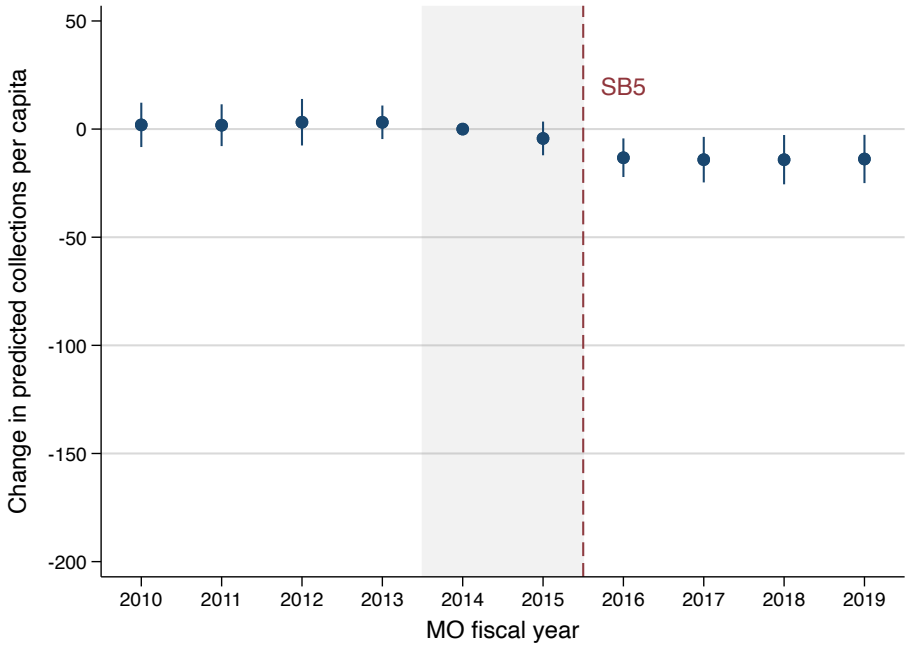
Notes: This table presents our baseline result, as well as a number of robustness checks meant to probe the relative importance of SB5 versus increased scrutiny triggered by the shooting of Michael Brown. Rows labeled "Collections" used collections per capita as the outcome variable. Panel A presents 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 that contain an interstate highway. Column (5) restricts to municipalities that were in the fourth quartile of collections per capita at baseline. Panel B presents results using alternative regression specifications. Column (1) presents our baseline results. Column (2) fully interacts the time fixed effects with the black population share as measured in the 2010 Census. Column (3) fully interacts the time fixed effects with the number of warrants issued at baseline. Column (4) 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 7: Ferguson effect versus SB5, counterfactual collections



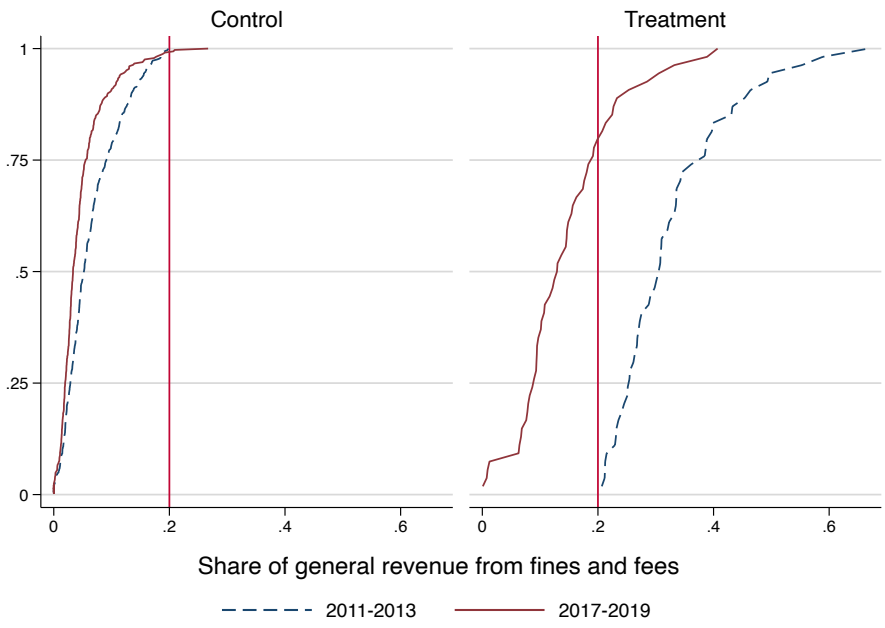
Notes: In this exhibit, we use the SB5 event study model to construct implied statewide counterfactual court collections under different scenarios. The x-axis of this figure represents fiscal years. The y-axis is per-capita court collections. The black line ("Raw Data") plots court collections over time 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 classify 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 8: Ferguson effect versus SB5, predicting collections with stops



Notes: This figure provides evidence that the post-SB5 declines in collections per capita is not fully predicted by the post-SB5 decline in stops. It 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 (3) 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 9: Decline in collections revenue among municipalities above the SB5 threshold



Notes: This exhibit provides evidence of the decline in collections revenue among municipalities above the 20% SB5 threshold. Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed collections to plot the cumulative distribution of court collections as a share of total general revenue. The left panel contains municipalities below the SB5 20% threshold on retained revenue at baseline (on average over 2011–2013) and the right panel contains municipalities at or above the 20% threshold at baseline. The dashed line gives the pre-SB5 distribution (averaged over 2011–2013), the solid lines gives the post-SB5 distribution (averaged over 2017–2019), and the vertical line denotes the 20% threshold. We find a large decline in the post-SB5 distribution of the court collections share for municipalities above the 20% threshold at baseline (right panel) but only a modest change for municipalities below the threshold (left panel).

Table 7: Share of revenue from fines and fees pre- and post-SB5

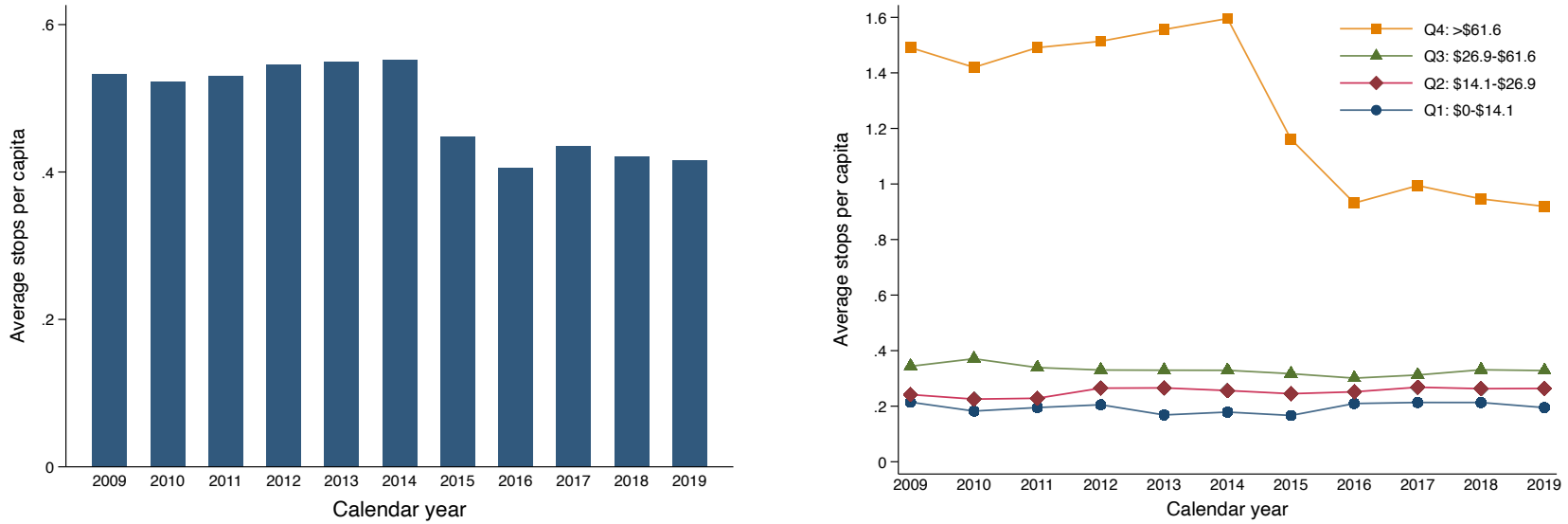
	Share of revenue from fines and fees		
	(1)	(2)	(3)
Treated Cts X Post SB5	-0.956***	-0.934***	-0.915***
	(0.111)	(0.115)	(0.122)
Observations	766	716	694
Pre-period mean	0.101	0.105	0.096
Municipalities	383	358	347
Adjusted R2	0.86	0.82	0.85

Notes: This table provides evidence that the cap of 20% is salient in the decline in collections post-SB5. Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed collections, excluding one municipality whose recorded collections exceeded total general revenue, and collapse the data to two observations per municipality, one for pre-SB5 (averaged over 2011–2013) and one for post-SB5 (averaged over 2017–2019). Each coefficient is generated from regressions using a continuous treatment of the form $Y_{mp} = \alpha_m + \delta_{ps(m)} + \beta \text{Post}_p \times C_m + \varepsilon_{mp}$ where Y_{mp} is court collections as a share of total general revenue in period p; 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); $\delta_{ps(m)}$ is a period by St. Louis area fixed effect; Post_p is an indicator for the post-SB5 period; and $C_m = \max(0, \text{share}_m - 0.2)$, which captures how much a municipality's pre-SB5 share exceeded the 20% cap, with C_m equal to zero for pre-SB5 shares below 20%. Column (1) contains our baseline results. Column (2) replaces the period by St. Louis area fixed $\delta_{ps(m)}$ with a more general county by period fixed effect. Column (3) restricts to municipalities with a population above 500. The pre-period mean is the average outcome among all municipalities pre-SB5. Standard errors are clustered at the municipality level and displayed in parentheses, with stars denoting statistical significance at the 1 (***), 5 (**), and 10% (*) levels. Each coefficient is close to -1, implying that for every percentage point above the cap in the pre-SB5 period, a municipality decreases their fines and fees share by a nearly equal amount in the post period, thus dropping down close to the cap of 20%.

A Online Appendix

A.1 Additional Figures and Tables

Figure A.1: Traffic stops declined after Ferguson and were concentrated in "high-fine" municipalities

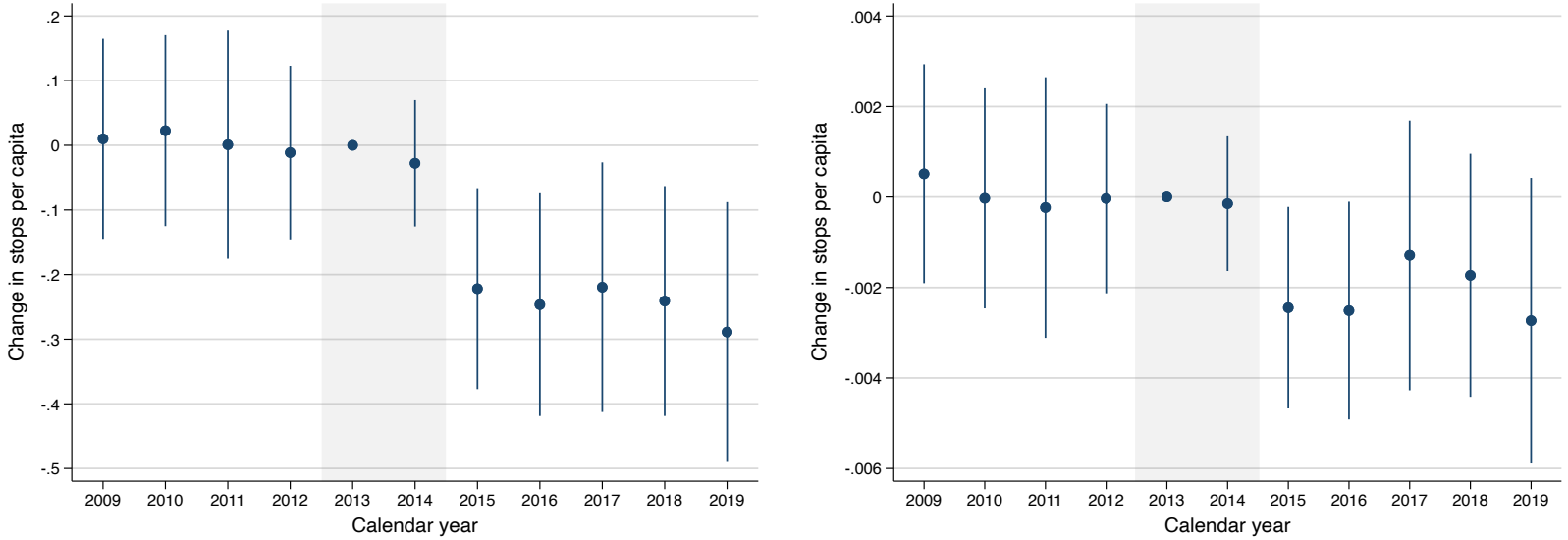


(a) Stops per capita by year

(b) Stops per resident by quartile

Notes: In this exhibit, we plot statewide trends for the average municipality in Missouri for traffic stops per resident and provide evidence that the declines occurred almost exclusively in municipalities that were raising excess revenue at baseline. Note that the x-axis is represented in calendar years to better align with the traffic stop data. The time-series (Panel A) exhibit significant declines (26% for stops) after the killing of Michael Brown in 2014 (fiscal year 2015). We then divide our sample of municipalities into four quartiles based on the distribution of collections per capita across fiscal years 2011–2013. The time series by quartile (Panel B) shows that the lowest three quartiles (Q1-Q3 in the figures) see little evidence of declines in traffic stops, but the highest quartile (Q4) sees large declines in the aftermath of the killing of Michael Brown in 2014 (fiscal year 2015).

Figure A.2: 'High-fine" and high-traffic municipalities also see relative declines in stops

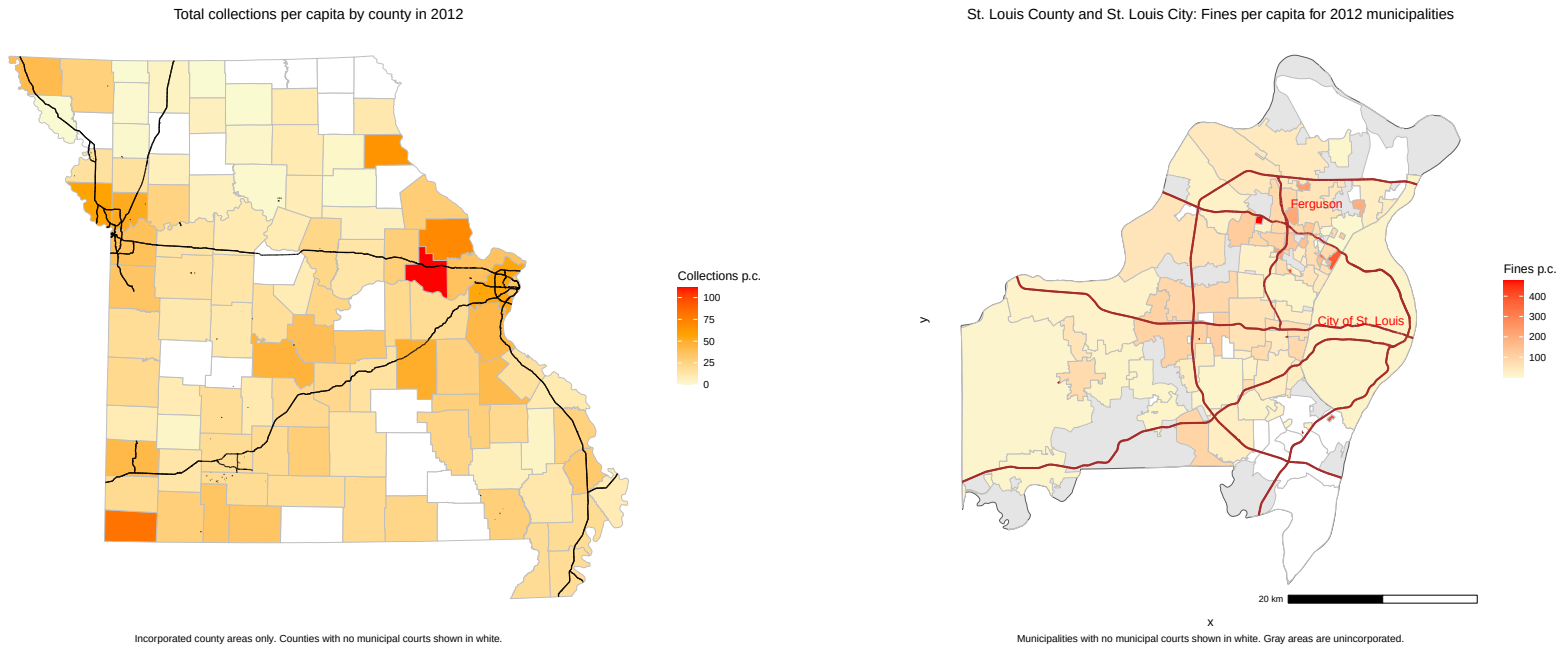


(a) Stops per resident (Q4 vs. Q1-Q3)

(b) Stops per resident (AADT)

Notes: This exhibit provides further evidence that the statewide declines in traffic stops occurred almost exclusively in municipalities that were best positioned to raise excessive revenue at baseline. Specifically, we use the universe of municipalities in Missouri with an active municipal court and are approximately balanced in terms of existence of observed stops to estimate models of the form: $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_\tau \times T_m + \varepsilon_{mt}$ where Y_{mt} is stops per capita in municipality m in calendar 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 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_m is the treatment measure. In Panel A, $T_m = \mathbb{1}_{m \in Q4}$, an indicator variable that takes a value of 1 for all municipalities who are in the fourth quartile based on the distribution of collections per capita over 2010–2012. In Panel B, T_m is the municipality's percentile in the distribution of average annual daily traffic (AADT) per capita over 2011–2013. Note that the x-axis is represented in calendar years to better align with the traffic stops 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 traffic stops in municipalities that were raising excessive revenue at baseline (Panel A) and with higher traffic volumes (Panel B).

Figure A.3: Interstate highways and collections in 2012

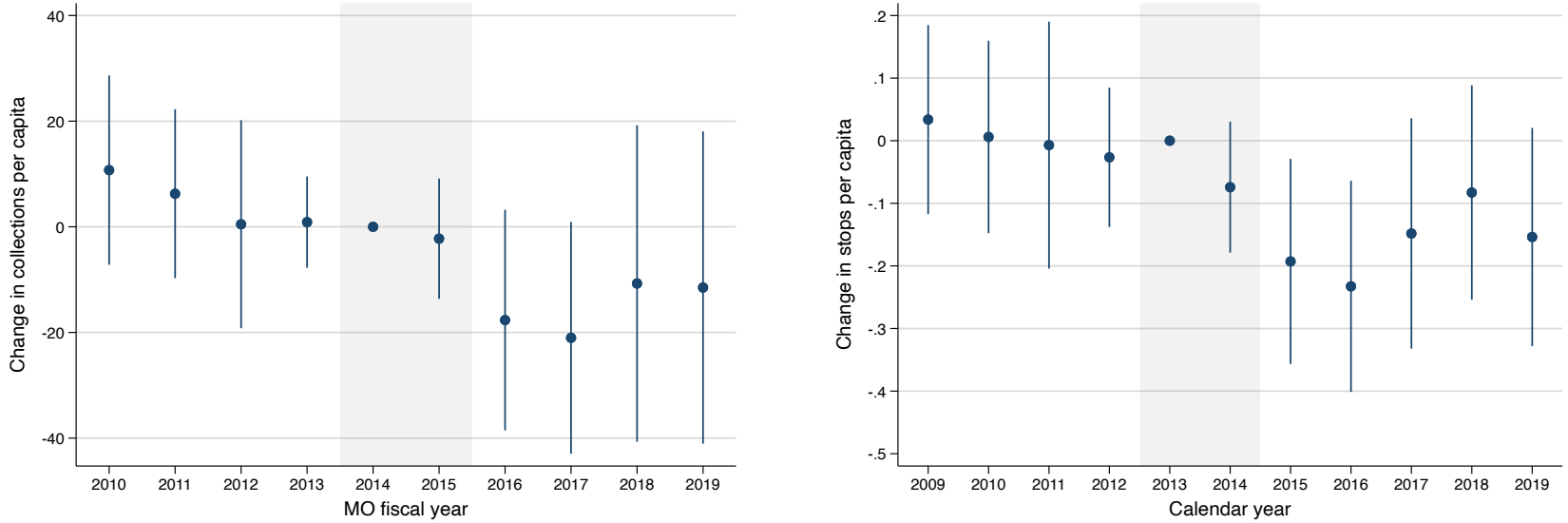


(a) Statewide map

(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.4: Interstate highway presence also predicts relative declines in collections and stops

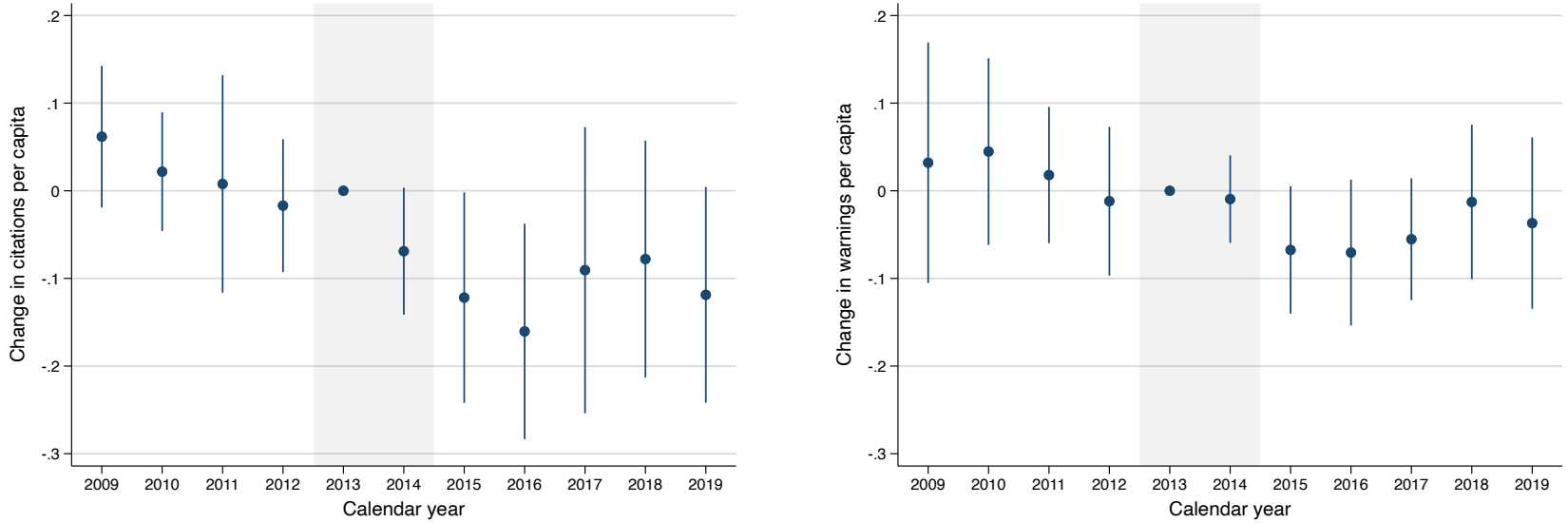


(a) Collections per capita

(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 ratio of interstate highway kilometers to square kilometers of land area in the municipality–see Appendix figure A.3 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 **??**). These findings are consistent with policing for profit via a "tax-exporting" motive as described in section 3.

Figure A.5: Interstate highway presence also predicts relative declines in stops that raise revenue



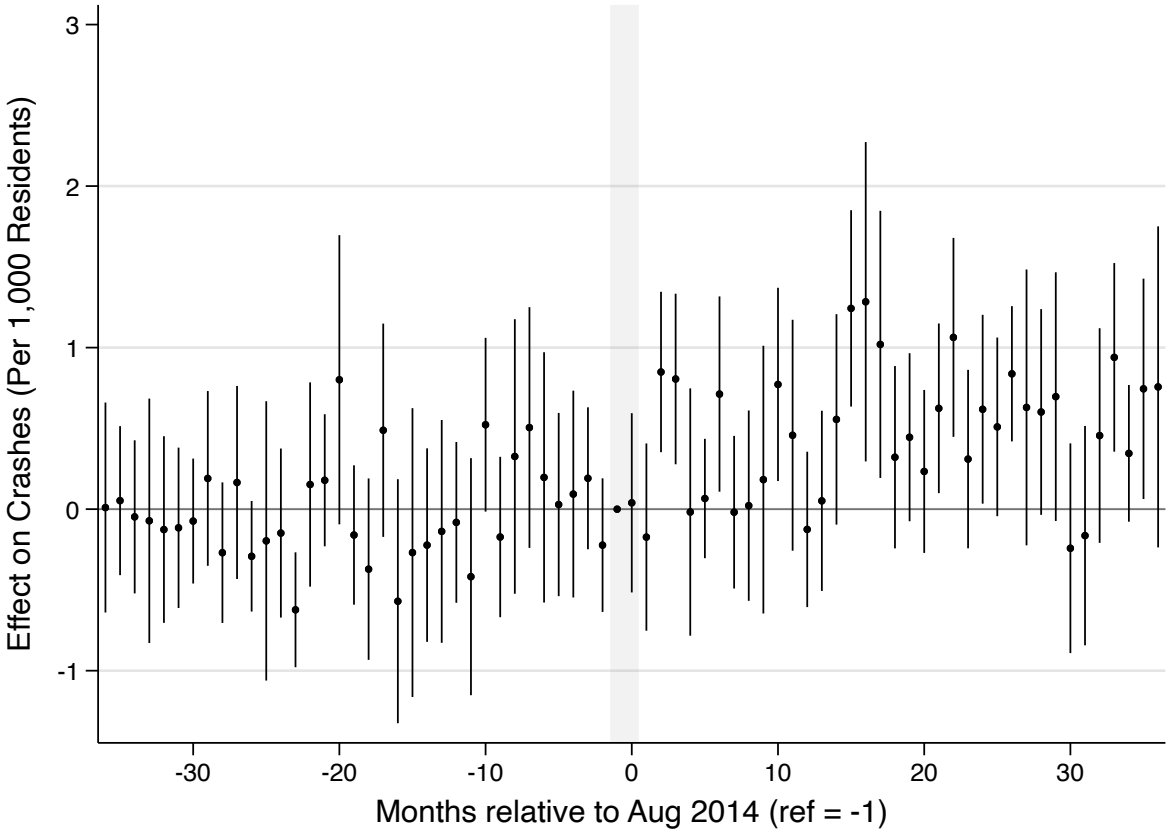
(a) Citations per capita

(b) Warnings per capita

Notes: This exhibit provides evidence that the statewide decline in traffic stops among municipalities that were likely engaged in policing for profit via the existence of extensive interstate highway networks coincide with a decline in stops that raise revenue . Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed stops 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 citations per capita (Panel A) or warnings per capita (Panel B) in municipality m in calendar 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 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); and X_m is the municipality's ratio of interstate highway kilometers to square kilometers of land area. Note that the x-axis is represented in calendar years to better align with the traffic stops 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. The results in this exhibit are noisier with evidence of larger relative declines in both citations and warnings in places with more interstate highway presence than those in Figure 3 which use collections and traffic (AADT) percentiles, our preferred measures of policing for profit.

Figure A.6: Baseline collections and safety – visual evidence

(a) Event study (Interstate)



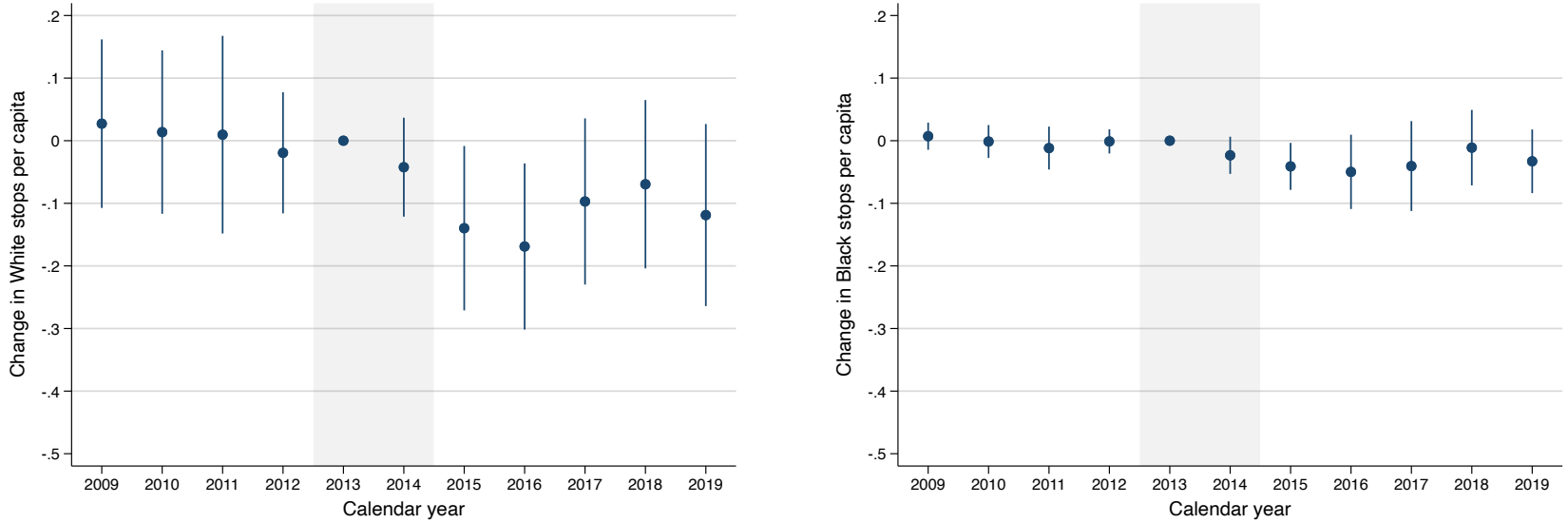
Notes: This figure presents event study coefficients from $Y_{mt} = \alpha_m + \delta_{ts(m)} + \sum_{\tau \in \mathbb{T}} \beta_\tau D_{m\tau} + \varepsilon_{mt}$ where Y_{mt} measures crashes per 1,000 residents on date t (month-by-year) in municipality m; α_m is a municipality fixed effect; $s = s(m)$ denotes whether municipality 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 killing of Michael Brown. Standard errors are clustered at the municipality level. Vertical lines denote 95% confidence intervals.

Table A.1: Baseline interstate highway presence and safety

	Panel A: All Roads (Outcome per 1,000 Residents)				
	(1)	(2)	(3)	(4)	(5)
Interstate Ratio X Post Ferguson	0.491***	0.955***	0.275***	0.282***	-0.009
	(0.111)	(0.218)	(0.096)	(0.099)	(0.010)
Outcome	Crashes	Vehicles	Injuries	Minor Inj.	Disabling Inj.
Observations	27521	27521	27521	27521	27521
Pre-period mean	1.350	2.469	0.436	0.393	0.043
Municipalities	377	377	377	377	377
Adjusted R2	0.69	0.70	0.23	0.22	0.05
	Panel B: Interstates Only (Outcome per 1,000 Residents)				
	(1)	(2)	(3)	(4)	(5)
Interstate Ratio X Post Ferguson	0.277***	0.550***	0.108*	0.112*	-0.004
	(0.069)	(0.151)	(0.056)	(0.059)	(0.006)
Outcome	Crashes	Vehicles	Injuries	Minor Inj.	Disabling Inj.
Observations	27521	27521	27521	27521	27521
Pre-period mean	0.190	0.334	0.056	0.051	0.005
Municipalities	377	377	377	377	377
Adjusted R2	0.65	0.67	0.19	0.18	0.03

Notes: This table presents further estimates on the impact of Ferguson on safety related outcomes. Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed stops, restricted to the 36 months before or after the killing of Michael Brown. Each number is generated from a regression of the form $Y_{mt} = \alpha_m + \delta_{ts(m)} + \beta D_t \times X_m + \varepsilon_{mt}$ 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; $D_t = 1(t - t^* > 0)$ is an an indicator that takes a value of 1 for every month after the killing of Michael Brown (t^*) in municipality m; and X_m is the municipality's ratio of interstate highway kilometers to square kilometers of land area. 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 A.6a. The pre-period mean is the average outcome among all municipalities before the killing of Michael Brown. Standard errors are clustered at the municipality level and displayed in parentheses, with stars denoting statistical significance at the 1 (***), 5(**), and 10% (*) levels.

Figure A.7: Interstate highway presence and decline in stops by driver race



(a) White stops per capita

(b) Black stops per capita

Notes: This exhibit shows that the decline in stops in municipalities with extensive interstate highway networks stems primarily from a decline in stops of White drivers, with little to no decline for Black drivers. Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed stops 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 stops per capita of White (Panel A) or Black drivers (Panel B) in municipality m in calendar 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 year by St. Louis area fixed effect; $D_\tau = 1(t - t^* = \tau)$ is an indicator that takes a value of one τ years relative to the killing of Michael Brown (where t^* = calendar year 2013); and X_m is the municipality's ratio of interstate highway kilometers to square kilometers of land area in the municipality. Note that the x-axis is represented in calendar years to better align with the traffic stops 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. The results in this exhibit are similar but noisier than those in Figures 5d and 5d, which use traffic (AADT), our preferred measure of policing for profit opportunity.

Table A.2: Baseline interstate highway presence and stop characteristics

	(1) Cites pc	(2) Warnings pc	(3) B stops pc	(4) W stops pc
Interstate Ratio X Post Ferguson	-0.121**	-0.059	-0.032	-0.112*
	(0.052)	(0.045)	(0.023)	(0.061)
Observations	4074	4074	4074	4074
Pre-period mean	0.290	0.210	0.056	0.420
Municipalities	377	377	377	377
Adjusted R2	0.79	0.78	0.58	0.85

Notes: This table presents further evidence that the decline in stops among municipalities with higher interstate highway presence—a measure of policing for profit opportunity—was concentrated in stops that raise revenue and stops of White drivers. Specifically, we use the universe of municipalities in Missouri with an active municipal court and that are approximately balanced in terms of existence of observed stops. Each coefficient is generated from a regression of the form $Y_{mt} = \alpha_m + \delta_{ts(m)} + \beta D_t \times X_m + \varepsilon_{mt}$ where Y_{mt} is either a stop outcome (citation or warning), or the count of stops by race of the driver (Black, White). The term 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; $D_t = 1(t - t^* > 0)$ is an an indicator that takes a value of 1 for every month after the killing of Michael Brown (t^*) in municipality m; and X_m is the municipality's ratio of interstate highway kilometers to square kilometers of land area. The dependent variables in columns 1 and 2 are citations and warnings per capita, and correspond to the event studies shown in Figures A.5a and A.5b. The dependent variables in columns 3 and 4 are stops of Black and White drivers per capita, and correspond to the event studies in Figures A.7b and A.7a. The pre-period mean is the average outcome among all municipalities before the killing of Michael Brown. Standard errors are clustered at the municipality level and displayed in parentheses, with stars denoting statistical significance at the 1 (***), 5 (**), and 10% (*) levels. The results in this table are similar but noisier than those in Table 4 and Table 5, which use collections and traffic (AADT), our preferred measures of policing for profit.

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	Q1-Q3	Q4
Crashes	1.842	1.333	1.030	2.288
Vehicles	3.438	2.454	1.898	4.203
Injuries	0.736	0.456	0.345	0.804
Minor Injuries	0.644	0.409	0.310	0.721
Disabling Inj.	0.093	0.047	0.036	0.083
	Panel B: Interstates Only (Outcome per 1,000 Residents)			
	All	Analysis	Q1-Q3	Q4
Crashes	0.337	0.168	0.082	0.439
Vehicles	0.662	0.302	0.143	0.801
Injuries	0.094	0.051	0.024	0.137
Minor Injuries	0.089	0.046	0.021	0.126
Disabling Inj.	0.004	0.005	0.003	0.010
Municipalities	470	373	283	90

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 baseline collections per capita in Q1-Q3. Column (3) corresponds to municipalities that have baseline collections per capita in Q4.

B.2 Policing Budgets and Workforce

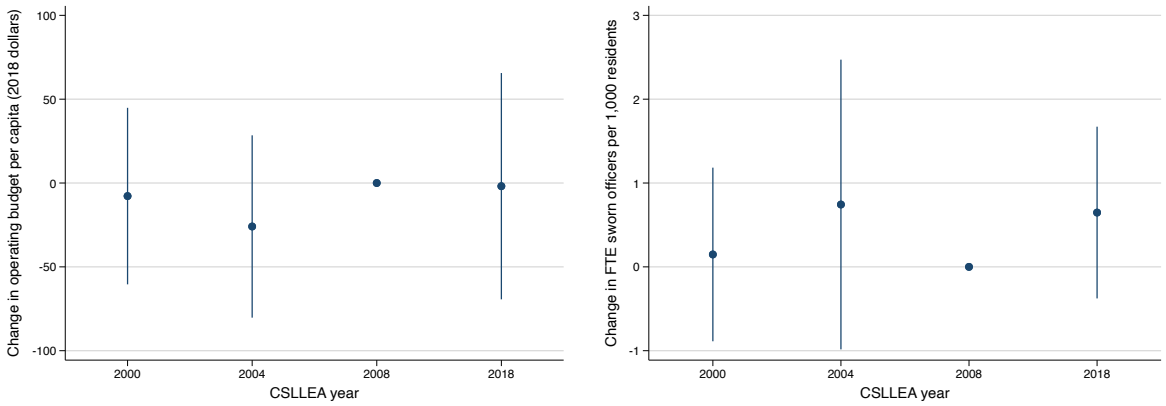
In this section we explore whether the observed decline in policing activity after SB5 could be the result of decreased funding for policing. We use data from the Census of State and Local Law Enforcement Agencies (CSLLEA) for 2000, 2004, 2008, and 2018 and focus on measures of police department operating budgets and the size of the workforce (officer counts).[38]. 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.

$$Y_{it} = \sum_{k \in T} \beta_k D_i \times \mathbf{1}(t = k) + \eta_i + \eta_t + \varepsilon_{it} \tag{4}$$

[38]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.8 we show event study plots for operating budget per capita and officers per 1000 residents, using a measure of full-time equivalent sworn officers.[39] 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.8: 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 4 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.8. 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.[40] 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.

[39] 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.

[40] 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 per 1,000	(4) Log officers
Treated X Post SB5	9.697	0.050	0.345	0.066
	(32.749)	(0.088)	(0.401)	(0.058)
Observations	1321	1322	1400	1403
Control mean	207.176	13.431	3.173	2.337
Municipalities	366	367	373	374
Adjusted R2	0.79	0.94	0.84	0.96

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.