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Fiscal Pressures and Discriminatory Policing: Evidence from Traffic Stops in Missouri

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Abstract

This paper provides evidence of racial variation in local governments' traffic enforcement responses to budget stress using data from policing agencies in the state of Missouri for the years 2001 through 2012. Like previous studies, we find that local budget stress is associated with higher citation rates. In addition, we find that there is an increase in traffic-stop arrests. However, we find that these effects are concentrated among white (rather than black or Hispanic) drivers. The results are robust to the inclusion of a range of covariates for traffic stops and to the inclusion of local population features interacted with year. The effect on citations holds in a regression-discontinuity specification looking at bare budget shortfalls. These results are consistent with a model where traffic police selectively target higher-income drivers to compensate for budget stress. Also consistent with this view, we find that the racial difference in citation rates is highest where the white-to-black income ratio is highest.

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1 Introduction

New models of policing combining aggressive tactics with data-driven management metrics have created tensions between the residents of "race-class subjugaged" (RCS) communities and police (Heymann 2000; Soss and Weaver 2017), with little evidence of substantial public safety gains (MacDonald et al. 2016). In this project, we examine the institutional determinants of racially targeted policing, where historically traffic and misdemeanor enforcement has been concentrated among drivers and residents in RCS communities. Our goal is to provide evidence on how the institutional preferences of law enforcement agencies influence disparities in policing activities, which could potentially lead to the reproduction of disadvantage among heavily policed populations.

A large literature has documented that traffic police disproportionately target black and Hispanic drivers when making stops. On the micro level, economic theorists have formalized conditions for measuring discrimination and econometricians have shown discrimination at the level of individual traffic stops (e.g. Knowles et al. 2001; Rojek et al. 2004). At the macro level, the consequences of this discrimination have been observed at the level of entire criminal justice systems, entire communities, and entire states (Fagan and Ash 2017; Baumgartner et al. 2018).

Another line of research has examined the public-finance motivations underlying aggressive policing. It is well-documented that local governments rely on revenue from traffic tickets, and officials often look to this source of revenue to help overcome budget shortfalls. In fact, some jurisdictions structure revenues that they anticipate from fines, fees and seizures into agency budgets (Baicker and Jacobson 2007). Other jurisdictions pursue these revenue-generating activities not only to provide municipal services, but to sustain their own police forces. For example, the recent Ferguson Report issued by the U.S. Department of Justice suggests that the municipality tried to cloak its taxing power in the exercise of police power by functionally equating the power of taxation with the power to punish (DOJ 2015). The report noted that local police in Ferguson and nearby communities had grown to depend on these revenue streams to sustain the size of the police force and to pay salaries and annual increases to the officers.

In this paper, we explore empirically the intersection of race, policing, and this form of latent taxation to shed light on how law enforcement's fiscal pressures interact with its treatment of members of different racial groups. We use data from the state of Missouri to assess whether police officers' ticketing behaviors are discriminatory, whether the disparity in ticketing changes when a municipality is faced with governmental pressures to increase ticketing revenue, and what these changes may suggest about Missouri law enforcement's racial punishment preferences.

To provide empirical evidence on these issues, we construct a data set on local fiscal stress for 196 local policing agencies in Missouri from 2000-2012. When local governments experience negative budgetary shocks (shortfalls), police may be given incentives to increase traffic enforcement to generate revenue (Garrett and Wagner 2009), or to shift resources to enforcement activities more likely to generate revenue. We use traffic enforcement and arrest data to assess the effects of fiscal pressure. Our innovation from the previous literature is to consider effects across different groups of drivers.

We find that at times of budget stress, local police and sheriffs increase their targeting of white drivers. Holding the number of traffic stops constant, the citation rate and arrest rate for white drivers increases. There is no effect on citations and arrests of non-white drivers. The finding is robust to a number of alternative specifications and checks, and it does not appear to be driven by confounding trends. The result holds in an regression-discontinuity (RD) framework where, if a locality barely has a revenue shortfall, there is a decrease in citation rates for blacks but an increase for whites.

These results may reflect a different set of institutional pressures regarding law enforcement and race than those that increase racial discrimination in response to political incentives such as the electoral cycle (Kubik and Moran 2003; Park 2014); instead, the results are consistent with a model where traffic police selectively target higher-income drivers to compensate for budget stress. Rather than being responsive to discriminatory pressures, the increased citation and arrest rates of white drivers may be indicative of a shift of resources towards targeting motorists who may be more likely to be able to pay fines. To probe this possibility, we examine whether the result depends on the ratio of incomes of white residents to black residents in the local community. Indeed, we find that the targeting of white drivers in citation and arrest rates is most prevalent where the white-to-black income ratio is highest.

Future research can provide further evidence on the reasons officers behave this way. We can ask whether officers are aware of white residents' higher average incomes, for example. In such a scenario, officers might typically discriminate against black and Hispanic drivers, as most research in this area suggests, but increase citations and arrests of white drivers only when "necessary" to optimize revenue.

2 Background

Racial disparities in traffic stops and citations are widespread in Missouri (Hernández-Murillo and Knowles 2004; Rojek et al. 2004; Rosenfeld et al. 2011) and elsewhere (Harris 1999; Epp et al. 2014; Baumgartner et al. 2018). Early work by economists on discriminatory enforcement in highway searches suggested two alternative explanations. Either police were stopping people of color more often because they were more likely to have drugs or contraband (statistical discrimination), or they were stopping these motorists more often based on racial preferences (taste-based discrimination) (Knowles et al. 2001; Persico and Todd 2006).

The interactions in this literature between racial preferences and varying pressure to maximize revenue are complex, an issue recently highlighted by the disproportionate burden of traffic enforcement fines on people of color that was a central focus of the Ferguson Report (DOJ 2015). These fines can grow into a broader range of fees that act as a latent tax on poor people (CEA 2015; Bannon et al. 2010; Harris 2017). To the extent that low-income motorists have less influence over local politics and policing than their high-income counterparts, systematic extractive stops as regressive tax policy may be consistent with political economy explanations of traffic enforcement discrimination (Makowsky and Stratmann 2009).

Monetary penalties have proven to be quite popular in state legislatures and in criminal legal institutions. Fines are seen both as a legitimate deterrent to wrongdoing and a means of transferring the costs of criminal justice administration (courts, police, prisons, etc.) to those accused of breaking the law, costs that would otherwise fall on ostensibly law-abiding taxpayers. In addition, unlike prison, fines do not keep the defendant out of the workforce.

Traffic stops can provide a politically expedient mechanism to generate revenue, since administrative fees allow state and local legislators to get around tough rules limiting local tax increases. Fines and administrative fees offer the executive a path to budgetary relief with limited legislative involvement or court oversight, allowing for de facto taxation by administrative rule making.

Recent studies, such as the DOJ Ferguson Report (DOJ 2015), are supportive of this instrumental motivation for police to pursue traffic stops: maximizing revenue to police agencies to sustain or expand police budgets. Police departments are often encouraged to maintain this revenue source at the expected level, and local executives have even reminded police departments that these revenues directly affect officers' pay. For example, in Figure 1 we include an infamous memo by Mayor John Gwaltney of Edmundson, Missouri, encouraging the local police department to write more tickets. In the letter, the mayor reminds the police department that "the tickets [officers] write do add to the revenue on which the [police department] budget is established and will directly affect pay adjustments at budget time."

[Figure 1 about here]

A related literature explores the broader political economy of revenue-focused policing and how such policies compromise social welfare. Garoupa and Klerman (2000) model the behavior of a rent-seeking government, suggesting that rent maximization may incentivize public officials to avoid deterrence of certain crimes and contrasting enforcement outcomes with that of a social-welfare focused government. Delhaye et al. (2007) analyze the ways that lobbying by differently situated publics influences the structure of enforcement of speeding laws. Makowsky and Stratmann (2009) use Massachusetts traffic citation data to find that officers' budget-maximizing behaviors are shaped by political considerations as well as personal preferences regarding race and gender. Even critics of the evidence for racial bias in revenue-focused policing acknowledge the pressure on local institutions to focus enforcement on those perceived as "outsiders" (Heriot 2017). And most recently, Goldstein et al. (2018) analyze data from 90,000 local governments and find that municipalities relying on fines and fees as a greater share of revenue have lower violent and property crime clearance rates, as police departments' energies are directed toward revenue-generating activities.

The Ferguson Report (2015) illustrates how this revenue-generating regime disproportionately penetrates communities with high proportions of people of color. Disparate racial treatment at each stage of processing in Ferguson skews the criminal justice "tax" toward people of color, whose economic position often is more tenuous than that of their white counterparts (Parker et al. 2010). The case of Ferguson is part of the broader geography of racial targeting in the aggressiveness of policing (Geller et al. 2014; Fagan and Ash 2017).

Ferguson also illustrates the ways that enforcement actions as minor as traffic citations can have cascading consequences for the socioeconomically disadvantaged. Traffic stops lead to tickets and fines, and the inability to pay those fines can lead to criminal arrests. Once arrested, the inability to post bail raises issues both before and after adjudication. Defendants charged with minor misdemeanors or outstanding warrants may have difficulty retaining counsel if required to pay a fee to establish indigency. The assignment of counsel may be delayed during the scramble to post bond between arrest and first appearance. The risk of fee default at that stage, which leads to pretrial delay or (worse) pretrial detention, creates additional risk of an adverse court outcome, both in terms of charging and sentencing. Failure to pay fees (which, as we said, can be seen as taxes) can prejudice these court outcomes and impose related burdens, including further monetary fines and criminal convictions. In effect, these regimes require defendants to pay fees and costs for the very court processes that lead to their punishment.

The use of arrest- or ticketing-generated revenues to offset budget shortfalls is hardly confined to Missouri (Sobol 2016). For example, Garrett and Wagner (2009) found that police in North Carolina issued more tickets after local revenue shortfalls, and Rowe (2010) found that discrimination against out-of-town drivers in traffic enforcement by police in Massachusetts is motivated by revenue shortfalls. Baicker and Jacobson (2007) showed that laws permitting police seizures of money incentivized police to increase drug arrest activities, leading to a tug-of-war between police and local public finance authorities. Surveying this literature, the Council of Economic Advisers (2015) concluded that "[i]ncreases in criminal justice spending have put a strain on local criminal justice budgets and led to the broader use of fine[s], penalties, and itemized criminal justice fees in an effort to support budgets."

Revenue generation is one of several incentives that criminal justice agents may respond to by increasing enforcement of criminal laws or shifting enforcement efforts to maximize productivity, and race often figures into that incentivized enforcement. For example, Gordon and Huber (2007) showed that when trial judges are up for election, they issue harsher criminal sentences, and Berdejo and Yuchtman (2013) showed that criminal sentences are 10% longer as judges approach the end of their electoral cycle. Park (2014) found that this electoral-pressure effect is disproportionately focused among black defendants. Relatedly, Kubik and Moran (2003) showed that states are approximately 25% more likely to conduct executions in gubernatorial election years than in other years, and that there is a larger effect on the probability that a black defendant will be executed than on the probability that a white defendant will be executed.

This paper aims to identify whether traffic police, like judges (Park 2014) and governors (Kubik and Moran 2003), respond to enforcement incentives in a way that is racially discriminatory. The Ferguson Report (2015) found evidence of fiscal enforcement motives within the courts, city government, and the police in particular; the interaction of these fiscal motives with racial preferences may provide additional insights about the structure of discriminatory policing. If police are responsive to fiscal pressures and aware that shifting resources away from racially targeted stops results in higher revenue, the consistent application of disproportionate enforcement pressure to people of color, whose economic position may make them less able and less likely to pay, raises questions.

One possibility is that the political economy of local policing makes it more costly for law enforcement to impose the latent taxation of consistent traffic citations on higher income motorists who may have more political influence; to the extent that officers use race as a proxy for this influence, they may target non-white drivers for consistent enforcement. Even if socioeconomically disadvantaged people of color have less ability to pay on average, targeting them consistently may maximize long run revenues if their relative political disenfranchisement (compared to whites) prevents them from effectively challenging discriminatory enforcement. Higher income, disproportionately white motorists may serve as a ready source of additional revenue in the short run, specifically in times of fiscal distress. Although the individual racial preferences of officers are likely to impact enforcement discrepancies (Donohue and Levitt 2001), revenue-maximizing policing may also be guided by these different revenue elasticities of enforcement that local officials expect to encounter among groups with varying political power (Makowsky et al. 2018).

3 Data

The paper merges two main datasets for the analysis. The first is the local government finances data for Missouri, from which we construct a measure of budget distress. The second is the agency-level traffic stops data, used to construct measures of traffic enforcement effort across racial groups. There are 769 agencies in the dataset, for which we have 13 years of annual panel data from 2000 through 2012. We also include a variety of municipal- and county-level census demographic measures.

The data on local government financial accounts come from the IndFin local government finances census dataset. This is a survey of all local governments administered every five years; if the localities do not provide previous years' data, those values are imputed by Bureau of Census statisticians. This induces measurement error but should not bias the estimates away from zero in either direction. The survey includes items on revenues, expenditures, assets, and liabilities. The data are matched to municipality governments (police departments) and county governments (sheriff's departments). Our preferred measure of local budgetary distress is based on Garrett and Wagner (2009). We have the log government revenue for agency i at year t, G_{it}. Our measure of fiscal health at year t is the proportional change in log revenue for the *previous* year, ΔG_{it-1}. This is meant to summarize the idea that there is a shortfall that is realized at the end of the year, which the government may try to make up for the next year through increased ticketing.

The data on traffic stops come from the Missouri Attorney General's Racial Profiling database. This is an annual survey of policing agencies that includes a distribution across race and ethnicity for all traffic policing actions. Missouri has been collecting statewide incident-level data on police traffic stops since 2001. Figure 2 shows the form that agencies have to fill out for every traffic stop. We have access to aggregate data, by agency and the race/ethnicity of the person stopped, for the years 2001 through 2013, and use the years 2001 through 2012. Hernández-Murillo and Knowles (2004), Rojek et al. (2004), and Rosenfeld et al. (2011) all have used these data to analyze aggregate racial disparities in traffic stops at different points in time.

[Figure 2 about here]

The merged traffic stop and finance data include over 700 of Missouri's counties and cities, while smaller municipalities, such as villages, are not included. We do not include these smaller municipalities, because they are difficult to merge with finance data (they may be less likely to respond to the IndFin survey, municipality names were less consistent for these locations, and quite a few municipalities cross county borders). However, these smaller municipalities have far fewer traffic stops than those included in the dataset, and they also typically have populations that are less diverse, racially. The local finance data are available for most of the sheriff's and police departments in the dataset for the years 2002, 2007, and 2012 when the IndFin survey was conducted, with more departments responding in 2007 and 2012 than in 2002. In non-survey years, we have finance data for 196 departments, including 69 sheriff's departments and 127 police departments.

The main variable of interest from IndFin is log revenue changes. We use the log revenue change for the previous year as a sign of fiscal health. The distribution of this variable is illustrated in Figure 3. We don't see any sign of manipulation of revenues around the zero cutoff.

[Figure 3 about here]

Finally, we collected and merged in a range of demographic variables from the American Community Survey (ACS), matchable to county or municipality. We use the ACS three-year estimates that span the time period included in this study. We use these as controls, interacted with year, and we also use them in heterogeneity analyses, as seen below.

We focus on four outcome variables constructed from the racial profiling data. First, we compute the **citation rate,** which is the number of citations issued by agency i to drivers of race r during year t, divided by the number of total traffic stops by agency i of drivers of race r during year t. Similarly, the **search rate** is the number of searches divided by the number of stops. The **hit rate** is the number of contraband discoveries divided by the number of searches. The **arrest rate** is the number of arrests divided by the number of stops. Summary statistics for these measures, by race, are reported in Table 1.

[Table 1 about here]

There are few differences by race or ethnicity in the citation rate. However, search and arrest rates are significantly higher for black and Hispanic motorists. The patterns in search and arrest rates are in line with those found nationwide; Black and Hispanic drivers are searched more frequently than white drivers and Hispanic drivers experience even higher search rates than blacks (Pierson et al. 2017). The hit rate is highest for whites, suggesting preferential treatment for whites in searches on average (e.g. Hernández-Murillo and Knowles 2004). To assess the statistical significance of these baseline differences, we estimate the following multivariate regression:

$$Y_{irt} = \alpha_{it} + \gamma_0 \text{Black}_{irt} + \gamma_1 \text{Hispanic}_{irt} + X'_{irt}\beta_{it} + \varepsilon_{irt} \tag{1}$$

where α_{it} is an agency-year fixed effect, Black_{irt} is a dummy variable equaling one for black drivers, and Hispanic_{irt} is a dummy variable equaling one for Hispanic drivers. We run this regression for black, Hispanic, and white drivers, so γ_0 and γ_1 give the average differences of blacks and Hispanics from whites, after residualizing out the fixed effects and controls.

We have access to a range of covariates, represented in X_{irt}, which again are aggregated by race. For demographics, we have age (proportion of drivers in bins 18-29, 30-39, and 40+) and gender (proportion male). We have the location (city-street, county road, interstate, state highway, U.S. highway) of the stop, reason for the stop (ex: moving violation), and the authority given for a search (consent, drug/alcohol odor, drug dog alert, incident to arrest, inventory, plain view, or reasonable suspicion). We also include the reason for arrest – drug violation, driving while intoxicated, assault, outstanding warrant, property crime, resisting arrest, and traffic violation.

The results from estimating Equation (1) are reported in Table 2. Black drivers tend to have a lower citation rate than white drivers, while Hispanic drivers are cited at a significantly higher rate. Both blacks and Hispanics are searched at a higher rate, with lower contraband hit rates, than whites. Both blacks and Hispanics are arrested at higher rates than whites. In particular, the lower rate of productive searches for people of color (Columns 5 and 6) suggests that police are more careful and selective in searching white motorists compared to non-white drivers (e.g. Hernández-Murillo and Knowles 2004).

[Table 2 about here]

4 Empirical Strategy

This section describes the approach for analyzing the relationship between local budget stress and discriminatory policing. The research design is based on that employed by Garrett and Wagner (2009), who found, using data from 1990 through 2003, that North Carolina municipalities with negative budget shocks responded by issuing more traffic tickets. The main goal, here, is to measure the disparate racial impacts of budget response by policing agencies.

We estimate the racial disparity in the change in enforcement outcome Y_{irt} (e.g., the citation rate) for agency i, race r, and year t using

$$\Delta Y_{irt} = \alpha_{ir} + \alpha_{rt} + \rho D_{it} + \rho_r R_r D_{it} + X'_{irt}\beta + \varepsilon_{irt} \qquad (2)$$

where α_{ir} is an agency-race interacted fixed effect, α_{rt} is a race-year interacted fixed effect, and ε_{irt} is an error term. We cluster standard errors by policing agency to allow for serial correlation across time in the agencies.

The treatment variable D_{it} is a measure for fiscal distress, defined as the negative change in revenue for the previous year in jurisdiction i. This is our preferred measure of local budgetary distress, based on Garrett and Wagner (2009). We expect, based on the previous paper, that $\hat{\rho} > 0$ for revenue-relevant enforcement actions; that is, fiscal distress (lower revenue growth) should be associated with higher growth in citations.

The term R_r is a dummy variable for the comparison race, and the term ρ_r gives the differential impact of lagged fiscal distress on race r. If local governments in budgetary distress seek to impose a larger share of taxes on members of racial minority groups, that would be consistent with $\hat{\rho}_r > 0$ for $r \in \{Black, Hispanic\}$. If instead local governments focus less enforcement on black and Hispanic drivers due to their typically lower income in response to budget distress, that would be consistent with $\hat{\rho}_r < 0$ for $r \in \{Black, Hispanic\}$.

The identification assumption for unbiased OLS estimates of ρ is that D_{it} is uncorrelated with other unobserved factors affecting traffic tickets in period t, conditional on the fixed effects. This may be a strong assumption if last year's budget conditions influence other socioeconomic and/or political factors this year that in turn affect traffic ticketing. An example of this type of factor would be decreases in expenditures on traffic lights and road signs, which may reduce ticketing. We assess the identification assumption by testing out different specifications and adding controls. In addition, we include a regression discontinuity specification where we look at discrete changes in policing around the budget shortfall threshold.

5 Results

This section reports our results. We consider a range of outcomes discussed in Section 3. We provide regression estimates for ρ and ρ_r in Equation (2) from Section 4. We report a number of specification checks, and then look at heterogeneous effects based on the characteristics of the jurisdictions.

The first regression estimates are reported in Table 3. We look at four outcomes: citation rate, search rate, hit rate, and arrest rate, defined in Section 3. The tables include our baseline specifications (with agency-race and race-year fixed effects) in Columns 1, 3, 5, and 7. The other columns (2, 4, 6, 8) include a number of stop-related covariates for the demographics of drivers arrested, and the reasons for stops, searches, and arrests, which may be correlated with driver race and subsequent outcomes. The first row is the baseline effect, while the second and third rows give the interacted effect for black and hispanic drivers respectively. Because the sample includes white, black, and hispanic drivers, the first row gives the average effect for white drivers.

We find, first, that a decrease in government revenue growth the previous year is associated with a higher citation rate, but only for white drivers (Columns 1 and 2). The coefficients for the interactions are negative; on Hispanic drivers it is not statistically significant and the coefficient on the interaction with black drivers is, but p is marginally insignificant with controls. There is a statistically significant increase on the arrest rate for white drivers as well, with the interactions insignificant. Finally, a decrease in government revenue growth appears to have no effect on the search rate or hit rate.

[Table 3 about here]

In Table 4 we look at the change in counts, rather than rates, to see what components of our variables are changing in response to the budget distress. First, we check whether it is driven by changes in total stops (versus changes in total citations, for example). We can see from Columns (1) and (2) that the results are not driven by change in total stops – these do not change in response to fiscal distress. The coefficients for white drivers on number of citations, searches, search hits, and arrests are all positive, but significant only for arrests. Meanwhile, the interacted effects on citations and searches for black drivers are negative and significant.

[Table 4 about here]

Overall, the results of the analysis of counts substantiate the main finding that fiscal distress changes the number of citations assigned to and number of arrests of white drivers. There is no effect on the number of traffic stops nor the interactions, meaning that officers are not replacing stops of black drivers with stops of white drivers. Rather they appear to be treating more harshly the same set of white drivers they normally stop. For black drivers, the fact that stops are not increasing means that the decrease in citations and searches cannot be interpreted as an expansion in the sample of stopped drivers to a marginal set (drivers with relatively lower criminality). Instead, it seems to be a re-allocation of policing time away from the same set.

Next, in Table 5 we further probe the results for citation rates. First, we present results from separate models for each racial group. We see that there is a positive relationship between fiscal distress and citation rate for white drivers (Column 1), but not for black (Column 2) or Hispanic (Column 3) drivers. The results for whites alone (Column 4), and from the model including all racial groups and a three-race interacted effect (Column 5), are robust to adding a set of pre-treatment census demographic controls (total population, percent white, percent urban, and percent over 65), interacted with a full set of indicators for each year in our data. In response to budget shortfalls, white drivers are getting more tickets, but black and Hispanic drivers are not.

[Table 5 about here]

Table 6 includes similar robustness checks for arrest rates. Again, we see a positive relationship between fiscal distress and arrest rates for whites, but not for black or Hispanic drivers. These coefficients are robust to the full set of census covariates interacted with each year in the data. Overall, these results support the view that in response to budget stress, Missouri police are arresting white drivers more often. An interpretation of this evidence is that white drivers would generate more revenue through legal financial obligations from arrest.

[Table 6 about here]

We report visual evidence of the relationship for citations in Figure 4, showing the RD jump for white, Black, and Hispanic drivers. The dependent variable, lagged change in log revenues, is on the x-axis. We mark the cut-off point at zero, with positive budget changes to the right and negative budget changes to the left. We can see a discrete change in citation activity at the zero revenue change cutoff. Just below the cutoff (a bare budget shortfall), we see a jump in citation rates for white drivers (left panel). For black drivers, there are slightly fewer citations below the cutoff (middle panel). For Hispanic drivers, there is no difference (right panel). When there is a revenue shortfall, police in the subsequent year tend to target white drivers with traffic citations.

[Figure 4 about here]

The regression results for this specification are in Table 7. First, we see in Columns 1 and 2 that the main result for citation rates holds for an alternative definition for budget stress: a dummy variable equaling one if revenues decreased in the previous year. The table shows that if reveues decreased, the citation rate increases for white drivers but not for black or Hispanic drivers. The effect of the dummy-variable treatment is robust to including the standard fiscal distress variable (lagged negative revenue change) as a control (Columns 3-4). Here, the coefficient for the fiscal distress variable is insignificant, meaning that the citation rate change is driven by the discrete budget-shortfall effect. For arrests, we see the opposite. There is no discrete jump in enforcements at the revenue-negative cutoff. Instead, it is driven by the continuous Fiscal Distress variable (Columns 7-8). The RD specificatin points to a causal relationship between budget shortfalls and racial differences in traffic citations.

[Table 7 about here]

Table 8 presents the results from our first heterogeneity analysis. One might wonder whether the effects of fiscal distress are concentrated in rural or urban areas, or whether they are concentrated in areas with relatively large populations of people of color. To make the categories referenced in the table, we divided the sample at the median values of urban density and racial makeup. First, we see that for both citations, and arrests, the effects are concentrated in rural areas. This makese sense, if these areas have more white residents than urban areas. Indeed, the increase in citations of white drivers in times of fiscal distress occurs in areas with smaller black populations. The increases in arrests of white drivers, however, is found in areas with larger black populations. This could be because RCS communities are more heavily and harshly policed than other communities, such that white residents found in those communities during times of fiscal distressed are the ones police target, for example.

[Table 8 about here]

Finally, Table 9 considers the importance of the white-black income ratio. Officers may increase citations and arrests of white drivers in response to fiscal distress because they expect that white drivers will have higher incomes than black or hispanic drivers and, therefore, be better able to pay the related fees, which will be used to address budgetary shortfalls. If this is the case, we should expect fiscal distress to lead to tougher treatment of stopped white drivers in areas where the income inequality between whites and blacks is highest. To test this, we split the sample by the white-black income ratio. A high ratio represents greater income inequality between whites and blacks.

For citation rates, we see that for both treatment specifications, and regardless of racial income inequality (Columns 1-4), there is a positive significant baseline effect on white drivers. However, the interacted negative effect for black drivers is larger in magnitude, more precisely estimated, and statistically significant only for areas with above-median white-black income inequality. This supports a model where officers are more likely to target white drivers in times of fiscal distress when they have a higher prior white drivers' ability to pay.

For arrests, we see an effect on white drivers only when there is a high white-black income ratio. The interactions with black driver are not significant. This could be more evidence that when whites are high-income, there is greater police targeting of whites for arrest in order to generate legal financial obligations. Given constraints on the total number of traffic stops that can be made, police agencies under fiscal distress re-allocate citations and arrests to these higher-income drivers.

[Table 9 about here]

6 Conclusion

The broad contribution of this project is the exploration of how local governments create incentives for law enforcement that contribute to the structure of discriminatory policing. While there is evidence of the relationship between local budget policies and police law enforcement practices, and a separate literature of racial discrimination in policing, this paper is the first to shed light on the interaction of these processes. We find that in response to budget distress, there is greater enforcement activity (ticketing and arrests) for white drivers, but not for non-white drivers. This result offers a different view of discriminatory enforcement than Park (2014), where judges responded to stronger enforcement incentives to administer punishment with greater discrimination: blacks were more likely to pay a racial punishment tax under pressure-incentive conditions than were whites (see Kennedy 1998).

There could be many mechanisms underlying the relationship uncovered here. One simple explanation for the marginal change in enforcement behavior is that police are aware of white drivers' greater ability to pay traffic tickets. When higher short-run revenue is necessary, officers shift their limited resources of time to increased targeting of white drivers. Consistent with this idea, we find that the racial differences in enforcement effects is highest in areas where there is a large white-to-black income ratio. These results offer less evidence of why Missouri police focus enforcement efforts on people of color in the absence of fiscal distress, despite lower hit rates among these motorists than white drivers and the fact that officers behave consistently with a belief that citations to white drivers generate more revenue.

Future work can shed further light on the factors contributing to the relationship between racial preferences and revenue incentives. Understanding how budget factors affect police discrimination, both in response to short-run fiscal shocks and in the aggregate, may suggest institutional solutions for reducing discrimination. Fiscally-pressured enforcement patterns may be valuable evidence of how officers behave in conditions where the incentive to produce race-neutral policing output crowds out unproductive racial preferences. These results raise important questions about the ways tensions in generating fine and fee revenue from RCS communities may be undergirded by political economy considerations. The broader social consequences of these processes are also uncertain. For example, future work may explore whether racially disparate budget effects have a subsequent impact on crime. The findings presented here highlight the complex relationship between local budgets, policing, and race as well as much that remains to be studied.

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Figures

Figure 1: Edmundson Mayor's Memo to Police Department Re Traffic Tickets

MEMO

Date: April 18, 2014

To: Edmundson P. D. – Sergeants and Patrolmen

Subject: Traffic tickets

In the past several weeks, the Board and I have noticed a marked downturn in traffic and other tickets being written by your department. It is correct that we have no quotas and want only "good tickets" written. However, we do have a record of your past performance to compare to your current performance and the picture that I see is a very disappointing one.

I wish to take this opportunity to remind you that the tickets that you write do add to the revenue on which the P. D. budget is established and will directly affect pay adjustments at budget time.

It is and has always been the desire of myself and the Board to provide a safe and pleasant work place with good compensation and benefits for everyone. However, our ability to continue doing this is being compromised by your work slow down. I realize that your work production records are directly affected by many extenuating circumstances and those factors are always accounted for as your work records are reviewed by myself and human resources.

As budget time approaches, 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.

Thank you

John E. Gwaltney

Mayor John Gwaltney

Figure 2: Vehicle Stop Information Form for Racial Profiling Database

VEHICLE STOP INFORMATION

DATE ☐ MM ☐ DD ☐ YY TIME ☐ ○ AM ○ PM

1 VIOLATION RESULTING IN STOP (✔ all that apply)

☐ MOVING ☐ EQUIPMENT ☐ LICENSE ☐ INVESTIGATIVE

If a "moving" violation, (✔ category of violation)

☐ SPEED ☐ LANE VIOLATION ☐ FOLLOW TOO CLOSE

☐ CVE ☐ FAIL TO SIGNAL ☐ OTHER MOVING VIOLATION

2 RESULT OF STOP (✔ all that apply)

☐ CITATION ☐ WARNING ☐ NO ACTION ☐ OTHER

3 DRIVER'S RACE/MINORITY STATUS (based **only** on visual observation)

☐ WHITE ☐ BLACK/AFRICAN-AMERICAN ☐ HISPANIC/LATINO

☐ AMERICAN INDIAN/ALASKA NATIVE ☐ ASIAN ☐ OTHER/UNKNOWN

4 DRIVER'S AGE ☐ UNDER 18 ☐ 18-29 ☐ 30-39 ☐ 40+

5 DRIVER'S GENDER ☐ MALE ☐ FEMALE

6 LOCATION OF STOP

☐ INTERSTATE HIGHWAY ☐ U.S. HIGHWAY ☐ STATE HIGHWAY

☐ COUNTY ROAD ☐ CITY STREET ☐ OTHER

7 WAS A SEARCH INITIATED? ☐ YES ☐ NO

If YES, probable cause/authority for search (✔ all that apply)

☐ CONSENT ☐ INVENTORY ☐ DRUG/ALCOHOL ODOR

☐ INCIDENT TO ARREST ☐ PLAIN VIEW CONTRABAND ☐ OTHER

☐ DRUG DOG ALERT ☐ REASONABLE SUSPICION-WEAPON (TERRY STOP)

8 WHAT WAS SEARCHED?

☐ DRIVER ONLY ☐ PROPERTY ONLY ☐ DRIVER AND PROPERTY

9 DURATION OF SEARCH

☐ 0-15 MINUTES ☐ 16-30 MIN. ☐ 31+ MIN.

10 WAS CONTRABAND DISCOVERED? ☐ YES ☐ NO

If YES, type of contraband (✔ all that apply)

☐ DRUGS/ALCOHOL/PARAPHERNALIA ☐ CURRENCY

☐ WEAPON ☐ STOLEN PROPERTY ☐ OTHER

11 WAS DRIVER ARRESTED? ☐ YES ☐ NO

12 IF ARREST MADE, CRIME/VIOLATION ALLEGED (✔ all that apply)

☐ OUTSTANDING WARRANT ☐ OFFENSE AGAINST PERSON

☐ RESISTING ARREST ☐ DRUG VIOLATION ☐ DWI/BAC

☐ PROPERTY CRIME ☐ TRAFFIC VIOLATION ☐ OTHER

Revised September 2004

VEHICLE STOP INFORMATION

DATE ☐ MM ☐ DD ☐ YY TIME ☐ ○ AM ○ PM

1 VIOLATION RESULTING IN STOP (✔ all that apply)

☐ MOVING ☐ EQUIPMENT ☐ LICENSE ☐ INVESTIGATIVE

If a "moving" violation, (✔ category of violation)

☐ SPEED ☐ LANE VIOLATION ☐ FOLLOW TOO CLOSE

☐ CVE ☐ FAIL TO SIGNAL ☐ OTHER MOVING VIOLATION

2 RESULT OF STOP (✔ all that apply)

☐ CITATION ☐ WARNING ☐ NO ACTION ☐ OTHER

3 DRIVER'S RACE/MINORITY STATUS (based **only** on visual observation)

☐ WHITE ☐ BLACK/AFRICAN-AMERICAN ☐ HISPANIC/LATINO

☐ AMERICAN INDIAN/ALASKA NATIVE ☐ ASIAN ☐ OTHER/UNKNOWN

4 DRIVER'S AGE ☐ UNDER 18 ☐ 18-29 ☐ 30-39 ☐ 40+

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☐ INTERSTATE HIGHWAY ☐ U.S. HIGHWAY ☐ STATE HIGHWAY

☐ COUNTY ROAD ☐ CITY STREET ☐ OTHER

7 WAS A SEARCH INITIATED? ☐ YES ☐ NO

If YES, probable cause/authority for search (✔ all that apply)

☐ CONSENT ☐ INVENTORY ☐ DRUG/ALCOHOL ODOR

☐ INCIDENT TO ARREST ☐ PLAIN VIEW CONTRABAND ☐ OTHER

☐ DRUG DOG ALERT ☐ REASONABLE SUSPICION-WEAPON (TERRY STOP)

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If YES, type of contraband (✔ all that apply)

☐ DRUGS/ALCOHOL/PARAPHERNALIA ☐ CURRENCY

☐ WEAPON ☐ STOLEN PROPERTY ☐ OTHER

11 WAS DRIVER ARRESTED? ☐ YES ☐ NO

12 IF ARREST MADE, CRIME/VIOLATION ALLEGED (✔ all that apply)

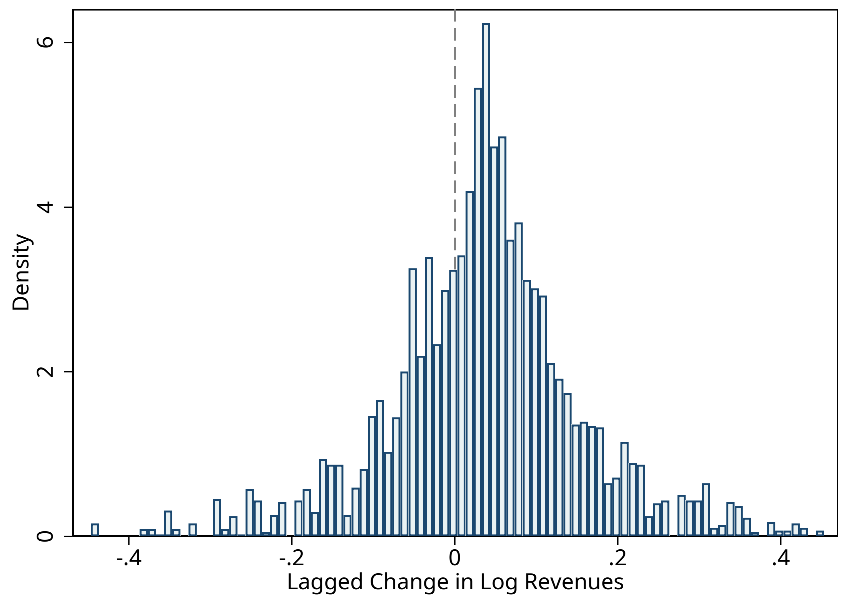
☐ OUTSTANDING WARRANT ☐ OFFENSE AGAINST PERSON

☐ RESISTING ARREST ☐ DRUG VIOLATION ☐ DWI/BAC

☐ PROPERTY CRIME ☐ TRAFFIC VIOLATION ☐ OTHER

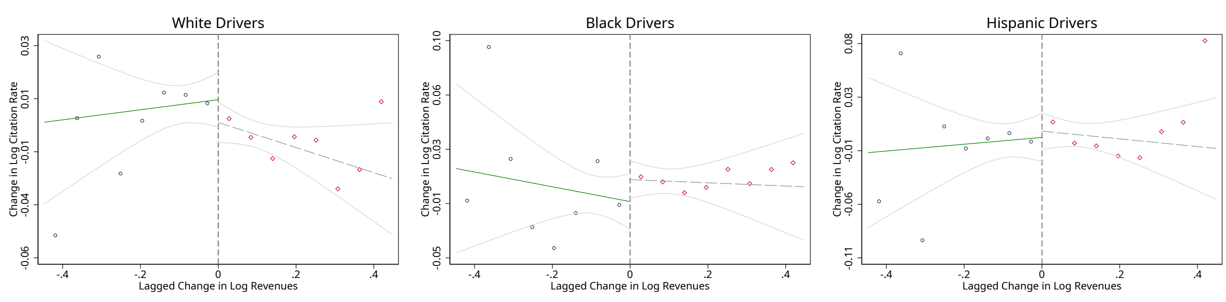
Revised September 2004

Figure 3: Distribution of Lagged Revenue Changes



Notes. Histogram of lagged log revenue changes from merged IndFin data. Bin width = .01. Vertical dashed line at zero.

Figure 4: Regression Discontinuity: Effect of Negative Revenue Change on Citation Rates by Race



RD visualization for effect of the lagged local revenue change (horizontal axis) on change in log citation rates (vertical axis), separately by race. Graphs produced by "cmogram" package in Stata.

Tables

Table 1: Summary Statistics on Stop Outcomes by Race

Race		Counts (by Agency-Year)					Rates			
		Stops	Citations	Searches	Hits	Arrests	Citation	Search	Hit	Arrest
Asian	Mean	33.732	20.67	1.074	.163	.744	.4778	.0414	.1625	.0299
	S.D.	144.842	99.041	5.402	.875	3.219	.4051	.1309	.3255	.1115
Black	Mean	450.598	295.789	51.634	9.092	38.642	.4649	.1105	.2236	.0826
	S.D.	2760.000	1780.000	321.317	56.630	237.992	.3275	.1434	.2963	.1325
Hispanic	Mean	60.874	38.622	7.977	1.170	5.675	.4984	.1361	.1686	.0960
	S.D.	394.418	285.101	41.714	6.478	29.628	.3493	.1886	.2753	.1637
Native American	Mean	6.076	3.198	0.506	.043	.114	.4898	.0936	.2309	.0704
	S.D.	21.899	12.817	1.868	.352	.563	.4471	.2292	.3858	.2005
White	Mean	1920.000	1090.000	124.945	28.843	86.646	.4636	.0797	.2790	.0543
	S.D.	12600.000	7970.000	629.777	159.255	469.428	.2733	.0829	.2287	.0723
Other	Mean	30.728	17.716	1.430	.257	.883	.4740	.0623	.19780	.0427
	S.D.	141.959	99.115	6.247	1.186	3.714	.3849	.1600	.3399	.1355
Total (All Races)	Mean	2144.987	1253.507	158.879	33.865	96.181				
	S.D.	14068.950	9069.148	822.011	189.017	561.344				

Table 2: Racial Differences in Stop Outcomes: Regression Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Citation Rate		Search Rate		Hit Rate		Arrest Rate	
Black Driver	-0.00700+	-0.0101**	0.0296**	0.00294**	-0.0343**	-0.0312**	0.0274**	0.00908**
	(0.00359)	(0.00376)	(0.00207)	(0.000886)	(0.00471)	(0.00582)	(0.00187)	(0.00135)
Hispanic Driver	0.0238**	0.0211**	0.0549**	0.00576**	-0.0894**	-0.0943**	0.0400**	0.0186**
	(0.00498)	(0.00583)	(0.00284)	(0.00116)	(0.00469)	(0.00897)	(0.00265)	(0.00201)
Agency-Year FE's	X	X	X	X	X	X	X	X
Demographics		X		X		X		X
Stop Reasons		X		X		X		X
Search Reasons				X		X		
Arrest Reasons								X
N	21777	21777	21802	21802	14216	14216	18591	18591
Adj. R^2	0.612	0.623	0.274	0.880	0.240	0.282	0.300	0.696

Notes. Observation is an agency-race-year, where whites, blacks, and Hispanics are included. Standard errors in parentheses, clustered by agency. + p<0.10, * p<0.05, ** p<0.01.

Table 3: Effect of Fiscal Distress on Enforcement Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ Citation Rate		Δ Search Rate		Δ Hit Rate		Δ Arrest Rate	
Fiscal Distress	0.0548**	0.0481*	0.0259+	0.0027	0.00239	-0.0076	0.0347**	0.0214*
	(0.0207)	(0.0195)	(0.0136)	(0.0104)	(0.0311)	(0.0321)	(0.0121)	(0.0096)
×Black Driver	-0.0693+	-0.0550	-0.0430	-0.0197	0.0107	0.0155	-0.0689+	-0.0390
	(0.0357)	(0.0361)	(0.0317)	(0.0285)	(0.0664)	(0.0655)	(0.0403)	(0.0270)
×Hispanic Driver	-0.0514	-0.0522	0.0581	0.0528+	0.0175	0.0343	0.0371	0.0154
	(0.0416)	(0.0414)	(0.0456)	(0.0279)	(0.0700)	(0.0682)	(0.0350)	(0.0257)
Agency-Race FE's	X	X	X	X	X	X	X	X
Race-Year FE's	X	X	X	X	X	X	X	X
Demographics		X		X		X		X
Stop Reasons		X		X		X		X
Search Reasons				X		X		
Arrest Reasons								X
N	3361	3361	3363	3363	2978	2978	2612	2612
R^2	0.115	0.193	0.067	0.505	0.103	0.154	0.088	0.497

Notes. Observation is an agency-race-year, where whites, blacks, and Hispanics are included. *Fiscal Distress* is defined as the log negative revenue change. ×*Black Driver* and ×*Hispanic Driver* indicate the interaction between *Fiscal Distress* and dummy variables for the respective driver race. Standard errors in parentheses, clustered by agency. + p<0.10, * p<0.05, ** p<0.01.

Table 4: Effect of Fiscal Distress on Enforcement Counts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Δ Log Total Stops		Δ Log Citations		Δ Log Searches		Δ Log Search Hits		Δ Log Arrests	
Fiscal Distress	-0.0674	-0.0845	0.143	0.127	0.214	0.0963	0.129	0.0697	0.521**	0.429**
	(0.102)	(0.102)	(0.124)	(0.123)	(0.160)	(0.141)	(0.153)	(0.153)	(0.162)	(0.149)
×Black Driver	-0.140	-0.167	-0.381**	-0.385**	-0.308*	-0.247+	-0.268	-0.285	-0.358+	-0.158
	(0.111)	(0.110)	(0.137)	(0.147)	(0.146)	(0.141)	(0.203)	(0.201)	(0.193)	(0.170)
×Hispanic Driver	0.0421	0.0189	-0.115	-0.165	0.0669	-0.0198	0.0513	-0.00282	-0.127	-0.197
	(0.148)	(0.146)	(0.170)	(0.174)	(0.209)	(0.199)	(0.227)	(0.220)	(0.232)	(0.237)
Agency-Race FE's	X	X	X	X	X	X	X	X	X	X
Race-Year FE's	X	X	X	X	X	X	X	X	X	X
Demographics		X		X		X		X		X
Stop Reasons		X		X		X		X		X
Search Reasons						X		X		
Arrest Reasons										X
N	3350	3350	3328	3328	3329	3317	3313	3308	2612	2609
R^2	0.114	0.139	0.121	0.144	0.092	0.255	0.089	0.154	0.108	0.271

Notes. Observation is an agency-race-year, where whites, blacks, and Hispanics are included. *Fiscal Distress* is defined as the log negative revenue change. ×*Black Driver* and ×*Hispanic Driver* indicate the interaction between *Fiscal Distress* and dummy variables for the respective driver race. Standard errors in parentheses, clustered by agency. + p<0.10, * p<0.05, ** p<0.01.

Table 5: Robustness: Effect of Fiscal Distress on Citation Rates

	(1)	(2)	(3)	(4)	(5)
	Effect on Δ Log Citation Rate				
Fiscal Distress	0.0471*	-0.0141	-0.00864	0.0492*	0.0510*
	(0.0207)	(0.0347)	(0.0421)	(0.0211)	(0.0197)
×Black Driver					-0.0620
					(0.0378)
×Hispanic Driver					-0.0603
					(0.0447)
Sample	Whites	Blacks	Hispanics	Whites	All
Agency-Race FE's	X	X	X	X	X
Race-Year FE's	X	X	X	X	X
Demographics	X	X	X	X	X
Stop Reasons	X	X	X	X	X
Census X Year FE's				X	X
N	1190	1109	1062	1159	3293
R^2	0.189	0.251	0.190	0.216	0.228

Notes. Observation is an agency-race-year, where whites, blacks, and Hispanics are included. *Fiscal Distress* is defined as the log negative revenue change. ×*Black Driver* and ×*Hispanic Driver* indicate the interaction between *Fiscal Distress* and dummy variables for the respective driver race. Standard errors in parentheses, clustered by agency. + p<0.10, * p<0.05, ** p<0.01.

Table 6: Robustness: Effect of Fiscal Distresss on Arrest Rates

	(1)	(2)	(3)	(4)	(5)
	Effect on Δ Log Arrest Rate				
Fiscal Distress	0.0223*	-0.00873	0.0311	0.0215*	0.0214*
	(0.00862)	(0.0212)	(0.0230)	(0.00890)	(0.00975)
×Black Driver					-0.0409
					(0.0278)
×Hispanic Driver					0.0132
					(0.0279)
Sample	Whites	Blacks	Hispanics	Whites	All
Agency-Year FE's	X	X	X	X	X
Race-Year FE's	X	X	X	X	X
Demographics	X	X	X	X	X
Stop Reasons	X	X	X	X	X
Census X Year FE's				X	X
N	919	854	836	919	2609
R^2	0.576	0.567	0.517	0.576	0.497

Notes. Observation is an agency-race-year, where whites, blacks, and Hispanics are included. *Fiscal Distress* is defined as the log negative revenue change. ×*Black Driver* and ×*Hispanic Driver* indicate the interaction between *Fiscal Distress* and dummy variables for the respective driver race. Standard errors in parentheses, clustered by agency. + p<0.10, * p<0.05, ** p<0.01.

Table 7: R.D. Specification: Revenue Reductions Vs. Budget Shorfall

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ Citation Rate				Δ Arrest Rate			
Sample	Whites	All	Whites	All	Whites	All	Whites	All
Budget Shortfall	0.0199**	0.0196**	0.0167+	0.0160+	0.00298	0.00388	-0.00572	-0.00559
	(0.00704)	(0.00673)	(0.00901)	(0.00887)	(0.00466)	(0.00477)	(0.00515)	(0.00531)
Budget Shortfall ×Black Driver		-0.0252*		-0.0207		-0.0188		-0.00987
		(0.0121)		(0.0143)		(0.0137)		(0.0144)
Budget Shortfall ×Hispanic Driver		-0.0162		-0.0118		0.0122		0.00597
		(0.0131)		(0.0174)		(0.0107)		(0.0131)
Fiscal Distress			0.0162	0.0186			0.0439**	0.0476**
			(0.0268)	(0.0272)			(0.0138)	(0.0143)
Fiscal Distress ×Black Driver				-0.0229				-0.0449
				(0.0393)				(0.0371)
Fiscal Distress ×Hispanic Driver				-0.0223				0.0327
				(0.0597)				(0.0520)
Agency-Race FE's	X	X	X	X	X	X	X	X
Race-Year FE's	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X
Census X Year FE's	X	X	X	X	X	X	X	X
N	1159	3293	1159	3293	895	2559	895	2559
R^2	0.146	0.177	0.146	0.177	0.113	0.122	0.120	0.125

Notes. Observation is an agency-race-year, where whites, blacks, and Hispanics are included. *Budget Shortfall* is defined as an indicator equaling one if revenue changes were negative last year.*Fiscal Distress* is defined as the log negative revenue change. ×*Black Driver* and ×*Hispanic Driver* indicate the interaction between the indicate revenue variable and the respective driver race. Standard errors in parentheses, clustered by agency. + p<0.10, * p<0.05, ** p<0.01.

Table 8: Heterogeneity by Urban Density and Race Makeup

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ Citation Rate		Δ Arrest Rate		Δ Citation Rate		Δ Arrest Rate	
Fiscal Distress	0.0704*	0.0247	0.0331+	0.00409	0.0669*	0.0285	0.0268+	0.0277*
	(0.0295)	(0.0210)	(0.0181)	(0.00681)	(0.0298)	(0.0230)	(0.0156)	(0.0138)
×Black Driver	-0.116+	-0.00117	-0.102*	0.0163	-0.0990	-0.0109	-0.0742	-0.0161
	(0.0644)	(0.0250)	(0.0480)	(0.0135)	(0.0654)	(0.0305)	(0.0506)	(0.0132)
×Hispanic Driver	-0.144*	0.0537	-0.00922	0.0112	-0.0664	-0.0637	0.00522	-0.0107
	(0.0714)	(0.0374)	(0.0469)	(0.0204)	(0.0696)	(0.0501)	(0.0404)	(0.0415)
Sample	Rural	Urban	Rural	Urban	<2.8% Black	>2.8% Black	<2.8% Black	>2.8% Black
Agency-Race FE's	X	X	X	X	X	X	X	X
Race-Year FE's	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X
Stop Reasons	X	X	X	X	X	X	X	X
Arrest Reasons			X	X			X	X
N	1769	1592	1289	1323	1696	1665	1291	1321
R^2	0.199	0.286	0.560	0.477	0.230	0.232	0.547	0.162

Notes. Observation is an agency-race-year, where whites, blacks, and Hispanics are included. *Fiscal Distress* is defined as the log negative revenue change. ×*Black Driver* and ×*Hispanic Driver* indicate the interaction between *Fiscal Distress* and dummy variables for the respective driver race. Standard errors in parentheses, clustered by agency. + p<0.10, * p<0.05, ** p<0.01.

Table 9: Heterogeneity by White-Black Income Ratio

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ Citation Rate				Δ Arrest Rate			
Fiscal Distress	0.0637+		0.0563*		0.00177		0.0508**	
	(0.0362)		(0.0258)		(0.0254)		(0.0184)	
Fiscal Distress ×Black Driver	-0.0787		-0.0969*		-0.0320		-0.108	
	(0.0602)		(0.0481)		(0.0376)		(0.0832)	
Budget Shortfall		0.0255*		0.0123+		-0.00653		0.00978*
		(0.0113)		(0.00735)		(0.00822)		(0.00432)
Budget Shortfall ×Black Driver		-0.0244		-0.0302*		-0.00427		-0.0282
		(0.0202)		(0.0148)		(0.0132)		(0.0236)
Sample	Low W-B Ratio		High W-B Ratio		Low W-B Ratio		High W-B Ratio	
Agency-Race FE's	X	X	X	X	X	X	X	X
Race-Year FE's	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X
Stop Reasons	X	X	X	X	X	X	X	X
Arrest Reasons					X	X	X	X
Census X Year FE's		X		X		X		X
N	1043	1043	1206	1206	824	824	911	911
R^2	0.211	0.213	0.252	0.253	0.188	0.190	0.124	0.124

Notes. Observation is an agency-race-year, where whites and blacks are included. *Fiscal Distress* is defined as the log negative revenue change. *Budget Shortfall* is defined as an indicator equaling one if revenue changes were negative last year. ×*Black Driver* and ×*Hispanic Driver* indicate the interaction between the indicate revenue variable and the respective driver race. Standard errors in parentheses, clustered by agency. Standard errors in parentheses, clustered by agency. + p<0.10, * p<0.05, ** p<0.01.

A Appendix

Table 10: Demographics of Agencies with and without Finance Data

	Mean Total Population		Mean Pct. White	
Year	Finance Data	No Finance Data	Finance Data	No Finance Data
2000	42320.09	2986.71	90.09	91.06
2012	72116.08	5011.89	81.51	93.13

Table 10 shows that our sample where data are matched tend to have the highest population (this is where IndFin does an annual, rather than five-year survey). The places with merged finance data have somewhat larger minority population shares.

In results available upon request, we also ran 2SLS regressions instrumenting fiscal distress with lagged inter-government revenues. There was still a positive effect of fiscal distress on white driver citation rates, but the interactions for black and Hispanic were insignificant. The main results are robust to weighting by 2000 population.