The Use and Effectiveness of Investigative Police Stops

Derek A. Epp
The University of Texas at Austin

Macey Erhardt
The University of Texas at Austin

This article asks if investigative police stops 1) help officers find contraband, and 2) serve as a bulwark against violent crime. We focus on the experiences of Fayetteville, North Carolina, which in 2012 mandated that police officers obtain written permission from motorists before conducting searches absent any probable cause. The effect of these mandates was a dramatic reduction is the use of so-called "consent searches." Using traffic stops data available from the North Carolina Department of Justice, we show that after these reforms went into effect officers made fewer overall searches, but contraband continued to be recovered at pre-reform levels, indicating a reduction in low-quality searches with minimal substantive impact. Moreover, we find that homicide rates are statistically indistinguishable between the pre- and post-reform periods. Thus, Fayetteville local government was able to implement community pleasing police reforms without jeopardizing community safety.

An investigative police stop occurs when an officer develops a suspicion that a person is involved in criminal activity and acts on this suspicion by detaining, conversing with, and possibly searching the person in question. What separates investigative stops from other types of police-initiated contact is that the stop itself is of secondary importance and is used simply as a means to bring an officer into closer contact with a person of interest. As such, investigative stops are often predicated on only trivial violations of the law (e.g. changing lanes without signaling or jaywalking), and for this reason are also referred to as pre-textual stops.

Investigative police stops rose to prominence during the 1980s and 1990s when they came to be regarded as a crucial element in efforts to crack down on violent crime, and drug crime in particular (Tyler, Jackson, and Mentovich 2015). The idea was that police officers would intensely patrol high-crime areas, maximizing police-citizen encounters, and in this way locate and remove as much contraband from targeted neighborhoods as possible (Wilson and Kelling 1982; Remsberg 1995). A famous application of this type of high-contact policing is the "broken windows" approach that was popularized in New York City in the 1990s. Legal justification for investigative stops stems from two Supreme Court cases: *Terry v. Ohio* (1968), which found that officers do not have to develop probable cause before momentarily detaining and frisking a pedestrian, and *Whren v. United States* (1996), a unanimous decision finding that officers can selectively stop motorists for traffic violations, no matter how trivial.

Whatever the intentions behind this policing strategy, one effect has been the creation of widespread racial disparities in policing outcomes (Baumgartner, Epp, and Shoub 2018; Epp, Maynard-Moody, and Haider-Markel 2014; Gelman, Fagan, and Kiss 2007; Peffley and Hurwitz 2010). In many jurisdictions, African Americans are more likely to be stopped and searched by the police than whites (Baumgartner et.al. 2017; Pierson et.al. 2017). Additionally, subjecting a segment of the population to a more intensive form of police scrutiny has been linked to a number of negative material, psychological, and social consequences (Alexander 2010; Burch 2013; Sampson and Loeffler 2010; Lerman and Weaver 2014a, 2014b; Howell 2009; Sances and You 2017).

Faced with mounting criticism, municipalities are rethinking their reliance on investigative stops and in some cases enacting policies to curtail their use. These policy changes give researchers leverage to evaluate the effectiveness of investigative stops by comparing periods of low and high usage, which is what we do in this article. We have two research questions: 1) how useful are these stops at recovering contraband, and 2) what is their relationship to violent crime rates. Together, these questions interrogate the central justification for investigative stops – finding contraband and reducing crime. To develop answers, we rely on traffic stops data maintained by North Carolina's Department of Justice (NCDOJ) and we focus on Fayetteville, NC – a medium-sized city that in 2012 placed restrictions on police consent searches, resulting in a dramatic reduction in their use. Consent searches are highly discretionary in that they do not require officers to have probable cause to search a motorist and are therefore similar to the "Terry stops" that are the basis of broken windows policies but for drivers instead of pedestrians.

Our findings are that reductions in consent searches are associated with markedly higher rates of discovering contraband, especially for searches of black drivers. In fact, not only do the rates improve, but Fayetteville officers are able to find similar levels of contraband even when conducting fewer overall searches, possibly because officers are able to spend more of their time developing probable cause and making higher-quality searches. Furthermore, using data from the FBI's Uniform Crime Reporting database, we find that consent searches appear to be unrelated to homicide rates. In summary, while investigate stops are known to generate racial differences, our findings suggest that they are relatively ineffectual tools for finding contraband and do not serve as a bulwark against violent crime as advertised.

This article builds most directly on scholarship by Mummolo (2018) who poses similar questions in the context of NYC's rapid dismantling of its stop-and-frisk program. His results are the same, finding that hit-rates improve after stop-and-frisk is scaled back, and no statistically meaningful connection with homicide rates. However, scholarship by Cassell and Fowles (2018) points in another direction as they attribute a recent spike in homicides in Chicago to reductions in Terry stops by that city's police force. Clearly then, more research on this subject is needed and our study makes some important additions to existing literature: we are the first to consider traffic stops, which in the wake of the *Whren* ruling have become the epicenter of police-citizen contacts (Eith and Durose 2011), we focus on a Southern city, and we are the first to focus on a medium-sized city with relatively lower rates of violent crime.

Our results are supportive of research suggesting that investigative stops may, if anything, be counterproductive. Epp et.al. (2014) argue that the primary result of these stops is to make motorists distrustful of the police, making it harder for officers to operate successfully in neighborhoods where investigative stops are concentrated. Similarly, cases of police violence have been shown to reduce public approval of the police (Jefferis et.al. 1997; Weitzer 2002) and dramatically reduce police-related 911 calls (Desmond et.al. 2016). Others have found conditional evidence of a "Ferguson effect" where crime rates increase in relation to perceptions that police legitimacy is compromised (Kane 2005; Pyrooz et.al. 2016). Clearly, any of these consequences are contrary to the aims of responsible police departments or city councils, but they are thought to be more likely in cities where over-policing has damaged community trust.

Municipalities, therefore, have good reason to wonder if scaling back consent searches and other types of investigative stops might actually yield safer communities. Evidence from Fayetteville suggests that they have little to lose.

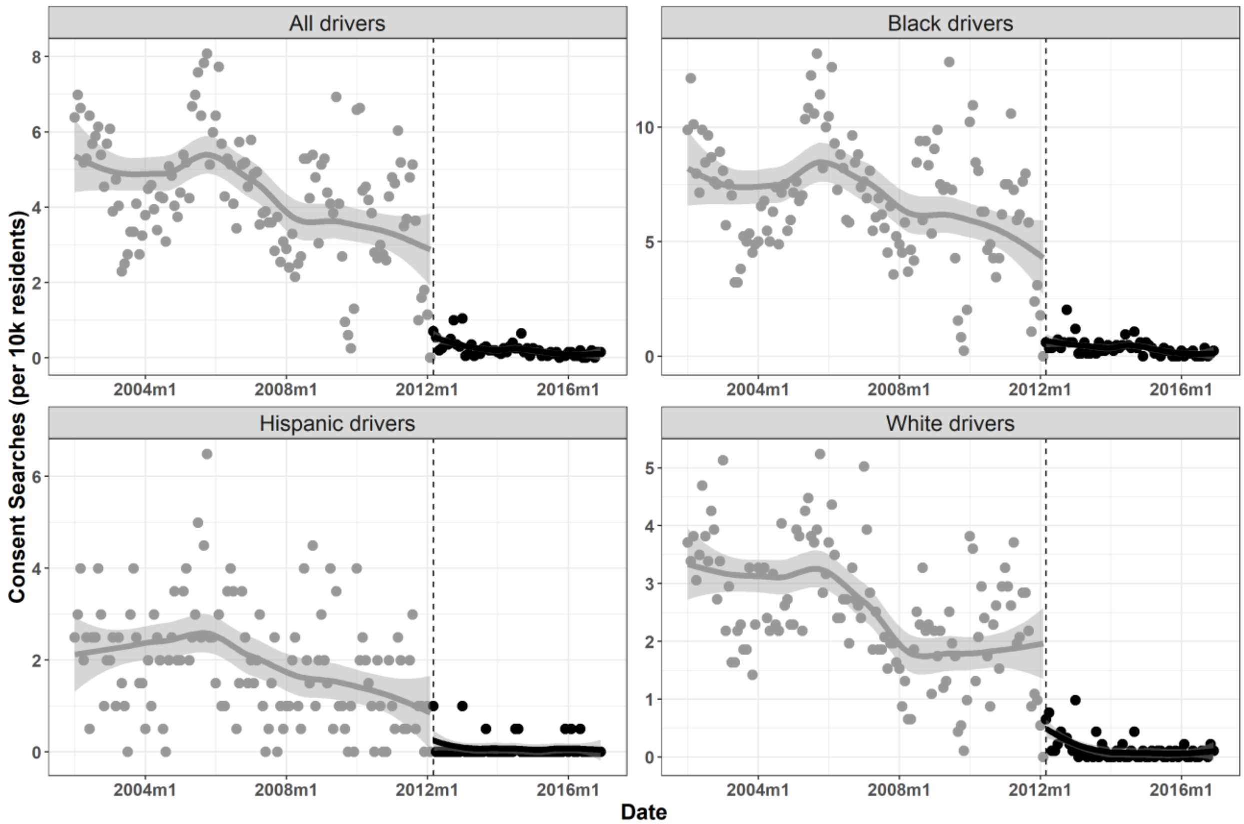
Data and context

Since 2002, police officers in NC have been required to fill out paperwork after pulling over a motorist, recording the reason the motorist was stopped and any subsequent enforcement actions that took place, including if the motorist was searched and if any contraband was recovered. Data collection is on-going and can be requested from the NCDOJ. Altogether, there is information on over 20 million traffic stops, making the NC database the most comprehensive record of police behavior in one state currently available.

We focus on traffic stops made by officers in Fayetteville, a city in central NC with approximately 210,000 residents of which 42% are black, 46% are white, and around 10% are Hispanic. From 2002 to 2016, Fayetteville officers pulled over 551,276 motorist and searched 26,341. Our interest in Fayetteville is motivated by local controversies over racial disparities in policing that emerged in the 2010s and eventually led to the implementation of policies requiring officers to obtain written consent from motorists before conducting consent searches. This added a substantial procedural barrier to carrying out consent searches as the written form made clear to motorists that they were within their rights to refuse the search. Prior to the controversy, Fayetteville officers had relied heavily on consent searches, but after the new written consent policy was in place, officers dramatically scaled back their use. Thus, our period of study spans a substantial overhaul in police behavior that deemphasized these highly discretionary searches.

The paperwork that NC officers are required to fill out distinguishes between five types of search: consent searches, probable cause searches, protective frisks, searches that take place incident to arrest, and warrant searches. Figure 1 shows the number of consent searches per 10,000 residents conducted by Fayetteville officers each month with separate panels for white, black, and Hispanic motorists.[1] The dashed vertical line is placed at March of 2012, when the city council reached an agreement with the police department to implement a written consent search policy (Barksdale 2010, 2011; Baumgartner, Epp and Shoub 2018).

Figure 1. Total monthly consent searches (per 10k residents), by race



Note: Dashed vertical line is at March 2012, indicating the start of the written consent search policy.

Fayetteville officers typically make between 100 and 300 searches of all types each month, corresponding to between 5 and 15 searches per 10,000 municipal residents. Figure 1 reveals that many of these overall monthly searches were consent searches. In fact, prior to the

[1] Rates are calculated on a monthly basis for visual clarity. Some days Fayetteville officers conduct only a few searches and no consent searches.

reform, 52% of total monthly searches were consent searches, on average. Thus, most of the searches being carried out by Fayetteville officers on local motorists lacked probable cause or a warrant. Taking all drivers together (the top-left panel), the average monthly search rate per 10,000 residents is 4.2. We then observe a sharp decline at the implementation of the written consent reform. In the post-reform period, the average monthly search rate is 0.22 and only 4% of total monthly searches are consent searches, on average.

It is fair to say that the written consent reform ushered in a new era of policing in Fayetteville, at least in regard to police searches. A persistent critique of consent searches is that the power dynamics of a traffic stop are such that motorists may find it difficult to deny an officer's search request, thus providing law enforcement with an avenue to circumvent the Fourth Amendment's restrictions on unreasonable searches. Written consent forms are thought to change this dynamic by making it clear to motorists that they have the right to refuse the search. Note that Fayetteville's reform did not forbid this type of search, which makes the sharp decline in the use of consent searches by Fayetteville officers highly suggestive. It seems that with the power dynamics of the stop altered, officers saw the utility of consent searches as greatly diminished, or, alternatively, motorists began exercising their right of refusal at a much higher rate. Whatever the case, it is noteworthy that the reform could produce such dramatic results simply by reminding motorists of their constitutional rights, indicating that the widespread use of consent searches requires motorists to be either ignorant of these rights or afraid to exercise them.

The other panels in Figure 1 isolate white, black, and Hispanic drivers. In each case, we observe the same pattern where the use of consent searches bottoms out almost immediately after the written consent policy is implemented. Notice, however, that the scale of the y-axis varies by race. On average, black motorists are twice as likely to experience a consent search as white motorists. In fact, officers conducted more consent searches of black motorists than white motorists in each of the 122 months that make up the pre-reform period, even though whites are a larger share of the Fayetteville population. Fayetteville's Hispanic population is smaller and so there are fewer stops and searches of Hispanics, resulting in a larger spread of points in the bottom-left plot. On average, Hispanics are less likely than either black or white motorists to experience a consent search, given their numbers in the population.

While traffic stops in Fayetteville clearly cover two distinct periods of police behavior, the policy change was too protracted to make causal attributions, which require a clean and ideally unexpected disjuncture. That was not the case for Fayetteville, where debate over the merits of moving to a written consent policy were debated in the press and by the city council for months. Officers were no doubt aware of this debate and it is possible that they made anticipatory changes in their searching behavior. Still, our analysis benefits from the wide variance in the use of consent searches by Fayetteville officers and the existence of a policy reform that clearly changed police behavior, even if it was not unexpected. Furthermore, the NC data is finely grained, with information on the success of every search conducted on a daily basis, so we can rely on a rich database of observations. Table 1 presents descriptive statistics on stops, searches, and contraband in Fayetteville to provide context for subsequent analysis.

Table 1. Traffic Stops, Searches, and Contraband in Fayetteville, 2002 – 2016

	All drives	White drivers	Black drivers	Hispanic drivers
# Stops	551,276	195,724	306,500	32,115
# searches	26,341	6,105	18,602	1,053
Search rate	4.78%	3.12%	6.07%	3.23%
# Contraband hit	7,140	1,534	5,228	215
Hit rate	27.11%	25.13%	28.10%	20.42%

Note: search numbers include all five types of search.

Between 2002 and 2016, Fayetteville officers made over 550,000 traffic stops, including 300,000 stops of black motorists, 200,000 stops of white motorists, and 32,000 stops of Hispanics. Approximately 5% of drivers are searched (taking all five search types together) and of those searched 27% are found to have contraband. Search rates vary widely by race (black motorists are almost twice as likely to be searched as white or Hispanic drivers) but racial differences are less pronounced when it comes to hit rates. Our analysis proceeds in two parts: first, considering the effectiveness of consent searches at finding contraband, and then assessing the relationship between consent searches and homicide rates.

Research Question 1: Do consent searches help the police find contraband?

We proceed by estimating the mean difference in daily contraband hit rates before and after the policy reform period, controlling for the previous day's hit rate, time trends, and, to account for any calendar effects, including indicators for the month and day of the week. Hit rates are calculated as the number of searches that resulted in contraband (i.e. the number of successful searches) divided by the total number of searches for that day. The models are specified as follows:

$$\text{Hit rate}_{(t)} = \beta 0 + \beta 1*\text{reform}_{(t)} + \beta 2*\text{time}_{(t)} + \beta 3*\text{time after reform}_{(t)} + \beta 4*\text{hit rate}_{(t-1)} + \beta m*\text{month}_{(t)} + \beta d*\text{day of week}_{(t)} + \varepsilon_{(t)}$$

Reform is an indicator coded 0 before the reform and 1 post-reform. *Time* is a continuous variable that counts the number of days from the start of the dataset (1 January 2002); *time after reform* counts the number of days from the start of the post-reform period (1 March 2012); β4 estimates the effect of the hit rate on the previous day; βm represents a series of estimates for months of the year; βd represents a series of estimates for days of the week. Table 2 shows the results, separately by the race of the driver. We present only the coefficients for the *reform* variable and the intercept, but full regression tables are available in the appendix (Tables 1A – 4A; pages 19 – 22).[2]

Table 2. Mean difference in daily hit rates after policy reform, 2002 – 2016

	Hit rates (All drivers)	Hit rates (White drivers)	Hit rates (Black drivers)	Hit rates (Hispanic drivers)
Reform period	7.43* (1.73)	5.19 (4.12)	6.28* (2.06)	-3.98 (16.61)
Intercept	17.38* (1.78)	18.05* (3.56)	15.71* (2.09)	28.03 (15.82)
N	4,770	2,107	4,333	173
R^2	0.065	0.024	0.065	0.123

* p-value ≤ 0.05
Note: Table shows OLS coefficients with robust standard errors in parentheses. Models include controls for time trends, the hit rate on the previous day, the month, and the day of week. The number of observations is the total number of days when a search was conducted.

Taking all drivers together, we find that after the policy reform went into effect overall daily hit rates increased by an average of 7%. This effect is statistically meaningful and substantively large. In fact, it represents a 43% increase over the pre-reform hit rate of 17%.[3] Hit rates are higher in the post-reform period for both white and black drivers, but the effect is only statistically significant for black drivers, suggesting that pre-reform consent searches of black motorists were of especially low-quality, at least when compared to other types of search that officers can conduct. Recall from Figure 1 that consent searches were used disproportionately on black motorists, so the results speak strongly to the types of "driving while black" effects documented by Epp et.al. (2014) whereby officers focus on black drivers for investigative stops with little to show for it in terms of substantive policing results. In the model isolating Hispanic drivers, the coefficient for the reform period is not statistically meaningful, possibly because there are only 173 days when a Hispanic driver was searched by a Fayetteville officer.

[2] The appendix re-estimates all subsequent analyses using difference of means tests without any controls (see Table 5A).
[3] The pre-reform hit rate is represented in the models by the intercept.

Thus, for Fayetteville, a dramatic reduction in the use of consent searches appears to have resulted in more accurate searching, at least of black motorists. However, part of the logic of investigative stops is that because they do not require the development of probable cause or the right contextual circumstances, they can be easily employed. In other words, the strategy is less about making high-quality searches than making a lot of searches, and the goal is simply to find as much contraband as possible. We therefore need to consider the possibility that while the reforms undertaken in Fayetteville improved hit rates, they may have resulted in less contraband being recovered overall if officers had to scale back the number of searches they could conduct.

To explore this possibility, we estimate the mean difference in the total number of daily searches and contraband hits before and after the policy reform went into effect. Once again, we control for time trends, the month, and the day of the week. However, for this iteration of models we control for either total searches or contraband hits on the previous day instead of the hit rate. Remember that when calculating hit rates, we divided total daily contraband hits by total searches. Our goal with this analysis is simply to explore changes in these compositional elements to better understand what is behind the increase in hit rates that we observed. Results are shown in Table 3.

Table 3. Mean difference in total daily searches and contraband hits after policy reform

	# searches	# contraband hits
Overall		
Reform period	-0.95* (0.18)	-0.07 (0.10)
Intercept	3.48* (0.22)	0.59* (0.09)
N	5,354	5,354
R^2	0.262	0.138
White drivers		
Reform period	-0.29* (0.07)	-0.04 (0.04)
Intercept	1.41* (0.08)	0.24* (0.03)
N	5,343	5,343
R^2	0.121	0.037
Black drivers		
Reform period	-0.70* (0.15)	-0.01 (0.08)
Intercept	2.24* (0.17)	0.33* (0.07)
N	5,350	5,350
R^2	0.211	0.114
Hispanic drivers		
Reform period	-0.08* (0.02)	-0.02 (0.01)
Intercept	0.27* (0.03)	0.04 (0.01)
N	5,027	5,027
R^2	0.024	0.008

* p-value ≤ 0.05

Note: Table shows OLS coefficients with robust standard errors in parentheses. Models include controls for time trends, the number of searches or contraband hits on the previous day, the month, and the day of the week.

The top rows show the results taking all drivers together. Here, we see that average number of searches conducted each day declines substantially in the post-reform period. Recall from Figure 1 that officers had at one time relied heavily on consent searches, so it is not surprising that overall daily searches would decline when this type of search became more difficult to implement. Of course, searching is a necessary prerequisite for finding contraband, and so we might expect that fewer searches would mean fewer contraband hits. However, when we look at the contraband model, we find that there is no statistically meaningful difference between the before and after periods. This means that officers are doing less searching after the reforms but to similar effect when it comes to finding contraband, suggesting that in the post-reform period officers are able to redirect time they would have spent on consent searches to more productive pursuits, such as developing probable cause. When we re-estimate the models after segmenting the data by the race of the driver, results are highly consistent. For whites, blacks, and Hispanics the post-reform period is associated with fewer daily searches, mirroring the results of the model taking all drivers together, and there is no statistically meaningful effect on contraband.

Do consent searches help police officers find contraband? Our findings suggest otherwise. Conducting fewer consent searches certainly improves hit rates. And this improvement appears to be driven by a reduction in low-quality searches in the post-reform period as contraband is recovered at similar levels even though fewer searches are being conducted. Given that consent searches (and other types of investigative stops) have been linked to a variety of negative social consequences, it is noteworthy that Fayetteville could dramatically curtail their use with few readily apparent consequences when it comes to finding contraband.

Research Question 2: Do consent searches prevent violent crime?

A frequent justification for investigative stops is their supposed role in preventing violent crime. While our findings suggest that consent searches are not very effective at recovering contraband (at least not in Fayetteville), it is possible that even unproductive searches when frequently conducted could act as a deterrent because criminals would know that the local police force is active and vigilant. To test this idea, we use homicide data available from the FBI's Uniform Crime Reporting database.[4] The advantage of focusing on homicides is that the official statistics are less dependent on levels of police attention than other types of crime. For example, official statistics of marijuana use are likely to depend heavily on how closely police officers are looking for marijuana users. If officers turn a blind eye, then very few cases of marijuana use would be

[4] Specifically, we use a database of these statistics compiled by Kaplan (2018).

reported. But the severity of homicides means that they are likely to gain official notice regardless of levels of police scrutiny.

FBI homicides statistics are available on a monthly basis. From 2002 to 2016, Fayetteville reported 138 total homicides and averaged 1.6 homicides month. Fayetteville has experienced rapid population growth and correspondingly more homicides over our period of study, and we therefore use homicides per 100,000 residents as our dependent variable. Table 4 reports the mean difference in these rates after the policy reforms were in effect. As with previous models, we control for time trends and included indicators for months. We also control for the previous month's homicide rate.

Table 4. Mean difference in monthly homicide rates after policy reform

	Homicide rate
Reform period	0.06 (0.23)
Intercept	1.03* (0.24)
N	165
R^2	0.064

* p-value ≤ 0.05

Note: Table shows OLS coefficients with robust standard errors in parentheses. The model includes controls for time trends, the month, and the homicide rate the previous month.

The reform period is not associated with a statistically significant coefficient, so it seems that reducing consent searches did not have any major effect on homicide rates. Of course, a caveat is that because homicide data is available only on a monthly basis this analysis is relatively low powered. As discussed, Cassell and Fowles (2018) link an uptick in homicides in Chicago to a reduction in the use of stop-and-frisk by Chicago police officers. They suggest that NYC – where Mummolo (2018) found no connection between stop-and-frisk and homicides – might prove to be an exception to a broader trend, but our findings indicate that the opposite is at least as likely. Fayetteville officers went from relying heavily on an investigative type of search to almost never using them without opening the doors to an increase in violence. Still, this is a question that merits further research. For example, Cassell and Fowles speculate that stop-and-frisk is more effective in cities that experience a lot of gun violence (like Chicago), which remains a possibility.[5]

Conclusion

Scholarship has long noted that the targeted nature of investigative stops leads to racial differences in policing outcomes with detrimental effects for black communities. The costs of such a policing strategy have been high. What, if any, are the benefits? Taking investigative stops on their own merits means acknowledging that they were never advertised as a nuanced approach to crime control, rather their virtues were to be found in their ubiquity. By maximizing contacts with residents of high-crime neighborhoods, officers could locate contraband and apprehend violent criminals through sheer force of numbers.

Our study of Fayetteville suggests that investigative stops come up short even by these standards. Naturally, by decreasing the number of searches conducted, there is a chance that less contraband will be found and taken off the streets. But our results highlight a different dynamic. We find that hit rates surged in Fayetteville after the implementation of a written consent search policy and that this change was driven by a decrease in the number of searches being conducted, with no corresponding drop in contraband. These findings point to a simple reality that officers have limited time on patrols, so obviously the question of how they spend that time is important. If they spend it conducting low-quality consent searches that means less time can be dedicated to developing probable cause. Frequent and fruitless searches of motorists can be deeply alienating,

[5] In the appendix (see Table 6A), we expand our analysis to include other types of violent crime using the FBI's Uniform Crime Reporting Database (ICPSR 2009-2016). As discussed, there are concerns about the reliability of crime statistics when reporting depends on levels of police attention. With this in mind, we find no evidence of an uptick in violent crime rates post reform.

so there is pressure on municipal governments to keep searches to a minimum. It is noteworthy that Fayetteville made progress toward this goal without sacrificing anything in terms of contraband recovery or community safety, as we found no evidence that investigative stops were serving as a bulwark against violent crime. Homicide rates are statistically indistinguishable from the pre- to the post-reform period.

High-quality data on police stops and their outcomes is rare and thus it is unusual to find jurisdictions where such data is available and where major police reforms have been implemented. New York City is one such jurisdiction and Fayetteville is another. Although these cities are demographically and geographically distinct, their experiences scaling back investigative police tactics were very much the same. This should give municipal governments confidence that they can move forward in rethinking the importance and effectiveness of these kinds of stops without risking dire consequences for their communities.

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Appendix

In the main article, we present abridged tables showing only the coefficients for the reform period and the intercept. What follows are the full regression tables displaying coefficients for all of the controls included in our models.

Table 1A. Full model results (Table 2)

	Hit rates (All drivers)		Hit rates (White drivers)		Hit rates (Black drivers)		Hit rates (Hispanic drivers)	
Reform	7.44*	(1.73)	5.19	(4.13)	6.28*	(2.06)	-3.97	(16.62)
Time	0.003*	(0.000)	0.003*	(0.000)	0.004*	(0.000)	0.003	(0.003)
Time after reform	-0.006*	(0.001)	-0.01*	(0.004)	-0.004*	(0.002)	0.002	(0.021)
Hit rate$_{(t-1)}$	0.06*	(0.01)	0.00	(0.02)	0.03	(0.02)	-0.09	(0.07)
January		-		-		-		-
February	1.63	(1.71)	4.20	(3.26)	0.99	(2.00)	-22.98	(13.67)
March	0.59	(1.70)	-0.39	(3.33)	0.00	(2.01)	-10.68	(15.27)
April	-0.68	(1.71)	-0.68	(3.55)	2.19	(2.16)	-9.37	(15.32)
May	0.37	(1.84)	2.22	(3.86)	-0.06	(2.20)	-3.46	(16.55)
June	0.52	(1.75)	2.81	(3.79)	-1.59	(2.04)	-6.81	(16.17)
July	-1.31	(1.76)	-0.73	(3.69)	-1.92	(2.08)	7.04	(20.63)
August	-3.24	(1.76)	0.78	(3.58)	-3.30	(2.11)	-20.58	(13.86)
September	-1.29	(1.78)	3.81	(3.64)	-1.77	(2.14)	-30.87*	(11.69)
October	-3.33	(1.77)	0.19	(3.51)	-3.14	(2.15)	-9.38	(15.22)
November	-1.87	(1.85)	-1.09	(3.55)	-3.89	(2.18)	-11.21	(15.96)
December	-1.17	(1.82)	4.36	(3.76)	-0.93	(2.22)	-32.35*	(12.20)
Sunday		-		-		-		-
Monday	-1.60	(1.45)	-2.75	(3.23)	1.28	(1.74)	8.09	(11.70)
Tuesday	-1.94	(1.44)	-0.90	(3.12)	-1.18	(1.69)	-12.85	(10.31)
Wednesday	0.07	(1.43)	-0.60	(3.07)	1.45	(1.66)	-5.63	(13.90)
Thursday	-0.22	(1.42)	-3.62	(2.97)	1.19	(1.66)	-4.73	(11.25)
Friday	-0.63	(1.41)	-4.48	(3.23)	1.18	(1.67)	2.41	(11.06)
Saturday	0.92	(1.48)	4.29	(3.23)	1.81	(1.73)	-3.89	(10.40)
Intercept	17.38*	(1.79)	18.06*	(3.56)	15.71*	(2.09)	28.04	(15.82)
N		4,770		2,107		4,333		173
R^2		0.065		0.024		0.065		0.123

* p-value ≤ 0.05

Note: Table shows OLS coefficients with robust standard errors in parentheses.

Table 2A. Full model results – searches (Table 3)

	Hit rates (All drivers)		Hit rates (White drivers)		Hit rates (Black drivers)		Hit rates (Hispanic drivers)	
Reform	-0.96*	(0.18)	-0.29*	(0.07)	-0.71*	(0.15)	-0.08*	(0.03)
Time	0.003*	(0.000)	-0.00	(0.00)	0.003*	(0.000)	0.00	(0.00)
Time after reform	-0.001*	(0.000)	-0.00	(0.00)	-0.001*	(0.000)	-0.00*	(0.00)
# $\text{Searches}_{(t-1)}$	0.28*	(0.01)	0.09*	(0.01)	0.24*	(0.01)	0.00	(0.01)
January		-		-		-		-
February	0.35	(0.23)	0.03	(0.09)	0.34	(0.18)	-0.01	(0.04)
March	0.01	(0.22)	-0.06	(0.09)	0.08	(0.18)	-0.03	(0.04)
April	-0.62*	(0.21)	-0.04*	(0.08)	-0.39*	(0.17)	0.06	(0.04)
May	-1.01*	(0.20)	-0.38*	(0.08)	-0.70*	(0.16)	-0.02	(0.04)
June	-0.70*	(0.21)	0.26*	(0.08)	-0.43*	(0.16)	-0.07*	(0.03)
July	-0.75*	(0.21)	-0.33*	(0.08)	-0.43*	(0.17)	-0.08*	(0.03)
August	-0.82*	(0.21)	-0.26*	(0.08)	-0.58*	(0.17)	-0.05	(0.03)
September	-0.68*	(0.22)	-0.17	(0.09)	-0.51*	(0.16)	-0.04	(0.04)
October	-0.64*	(0.21)	-0.20*	(0.08)	-0.48*	(0.17)	-0.03	(0.04)
November	-0.89*	(0.21)	-0.28*	(0.08)	-0.66*	(0.16)	-0.04	(0.03)
December	-0.91*	(0.21)	-0.27*	(0.08)	-0.63*	(0.16)	-0.76*	(0.03)
Sunday		-		-		-		-
Monday	0.42*	(0.15)	0.04	(0.06)	0.35*	(0.12)	-0.17	(.03)
Tuesday	0.69*	(0.15)	0.19*	(0.06)	0.49*	(0.12)	-0.02	(.02)
Wednesday	0.94*	(0.15)	0.17*	(0.06)	0.81*	(0.12)	-0.03	(.02)
Thursday	1.07*	(0.16)	0.23*	(0.06)	0.84*	(0.13)	0.02	(.03)
Friday	0.73*	(0.15)	0.23*	(0.06)	0.58*	(0.12)	-0.01	(.03)
Saturday	0.04	(0.14)	-0.02	(0.06)	0.10	(0.11)	0.01	(.03)
Intercept	3.49*	(0.23)	1.42*	(.09)	2.24*	(0.17)	0.27*	(0.04)
N		5,354		5,343		5,350		5,027
R^2		0.262		0.121		0.21		0.024

* p-value ≤ 0.05

Note: Table shows OLS coefficients with robust standard errors in parentheses.

Table 3A. Full model results – contraband hits (Table 3)

	Hit rates (All drivers)		Hit rates (White drivers)		Hit rates (Black drivers)		Hit rates (Hispanic drivers)	
Reform	-0.07	(0.11)	-0.04	(0.04)	-0.01	(0.09)	-0.02	(0.02)
Time	0.003*	(0.000)	0.00*	(0.00)	0.00*	(0.00)	0.00*	(0.00)
Time after reform	-0.001	(0.000)	-0.00	(0.00)	-0.00*	(0.00)	-0.00*	(0.00)
# $\text{Hits}_{(t-1)}$	0.18*	(0.02)	0.04*	(0.02)	0.14*	(0.02)	0.00	(0.02)
January		-		-		-		-
February	0.15	(0.11)	0.07	(0.04)	0.14	(0.09)	-0.04	(0.16)
March	0.07	(0.11)	0.05	(0.04)	0.05	(0.09)	-0.01	(0.02)
April	-0.14	(0.10)	-0.07	(0.04)	-0.05	(0.07)	-0.01	(0.02)
May	-0.26*	(0.09)	-0.06	(0.04)	-0.19*	(0.08)	-0.01	(0.02)
June	-0.19*	(0.09)	-0.02	(0.04)	-0.15	(0.08)	-0.02	(0.02)
July	-0.31*	(0.09)	-0.07*	(0.04)	-0.20*	(0.08)	-0.03*	(0.02)
August	-0.31*	(0.09)	-0.08*	(0.04)	-0.21*	(0.08)	-0.02	(0.02)
September	-0.26*	(0.01)	-0.02	(0.04)	-0.22*	(0.08)	-0.01	(0.02)
October	-0.32*	(0.09)	-0.06	(0.04)	-0.23*	(0.08)	-0.03	(0.17)
November	-0.34	(0.09)	0.05	(0.04)	-0.26*	(0.08)	-0.03	(0.02)
December	-0.33	(0.09)	-0.05	(0.04)	-0.23*	(0.08)	-0.05*	(0.02)
Sunday		-		-		-		-
Monday	0.05	(0.06)	-0.01	(.03)	0.06	(0.06)	-0.01	(.01)
Tuesday	0.10	(0.07)	0.04	(.03)	0.08	(0.06)	-0.02	(.01)
Wednesday	0.31*	(0.07)	0.04	(.03)	0.26*	(0.06)	0.00	(.01)
Thursday	0.28*	(0.08)	0.04	(.03)	0.25*	(0.06)	0.00	(.01)
Friday	0.23*	(0.07)	0.04	(.03)	0.21*	(0.06)	-0.01	(.01)
Saturday	0.07	(0.07)	0.02	(.03)	0.06	(0.06)	-0.01	(.01)
Intercept	0.60*	(0.09)	0.24*	(0.04)	0.34*	(0.08)	0.05*	(0.02)
N		5,354		5,343		5,350		5,027
R^2		0.138		0.037		0.114		0.008

* p-value ≤ 0.05

Note: Table shows OLS coefficients with robust standard errors in parentheses.

Table 4A. Full model results (Table 4)

	Homicide rate
Reform	0.06 (0.24)
Time	-0.00 (0.00)
Time after reform	0.00 (0.00)
# Homicide rate$_{(t-1)}$	-0.02 (0.08)
January	-
February	-
March	-0.23 (0.25)
April	0.12 (0.28)
May	0.05 (0.25)
June	0.43 (0.34)
July	0.05 (0.23)
August	0.01 (0.29)
September	0.05 (0.26)
October	-0.15 (0.26)
November	0.28 (0.31)
December	-0.15 (0.29)
Intercept	1.03 (.24)
N	165
R^2	0.064

* p-value ≤ 0.05

Note: Table shows OLS coefficients with robust standard errors in parentheses.

Table 5A shows the results of difference of means test between the pre- and post-reform periods. In each case, the post-reform period is associated with substantially higher hit rates and fewer overall searches, replicating results from the models in the main article. However, results diverge when it comes to overall contraband hits, where difference of means tests indicate a reduction in the amount of contraband recovered taking all drivers together and for white and Hispanic drivers. There is no difference in contraband recovery for black drivers between the pre- and post-period. We believe that the models presented in the main article are better specified, but these results do suggest the possibility that the written consent policy had different effects depending on the race of the driver. One possibility that would be consistent with these findings is if Fayetteville police were more likely to make low-quality consent searches of black motorists than of whites or Hispanics. We also find no evidence of a meaningful difference in homicides rates, replicating results from Table 4.

Table 5A. Mean differences (pre-reform mean – post-reform mean)

Variable	Difference (overall)	Difference (white drivers)	Difference (black drivers)	Difference (Hispanic drivers)
Hit rate	-11.99* (0.80)	-5.89* (1.55)	-13.84* (0.93)	-9.31* (3.18)
# searches	2.49* (0.09)	0.79* (0.03)	1.52* (0.07)	0.13* (0.01)
# contraband hits	0.22* (0.04)	0.14* (0.01)	0.05 (0.03)	0.01* (0.00)
Homicide rate	0.05 (0.12)	-	-	-

* p-value ≤ 0.05
Note: entries are the mean of the post-reform period subtracted from the mean of the pre-reform period.

Table 6A shows the results of a difference of means test in violent crime rates, comparing the pre- and post-reform period. Crime data for Fayetteville is available on a monthly basis from the FBI's Uniform Crime Reporting Program Database (Offenses Known and Clearances by Arrest) from 2009 to 2016 (ICPSR 2009-2016). Violent crimes are murders, manslaughter, rapes, robberies, and assaults. We use a difference of means test instead of a fully specified OLS regression because there are only 81 total observations available. Statistically, there is no difference in violent crime rates after the written consent reform went into effect.

Table 6A. Mean difference in monthly violent crime rates (pre-reform mean – post-reform mean)

	Crime rate (Fayetteville)
Crime rate	-0.08 (8.86)
N	81

* p-value ≤ 0.05
Note: entries are the mean violent crime rate of the post-reform period subtracted from the mean of the pre-reform period.