Was there a Ferguson Effect on crime rates in large U.S. cities?

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WAS THERE A FERGUSON EFFECT ON CRIME RATES IN LARGE U.S. CITIES?[1]

ABSTRACT

Purpose: There has been widespread speculation that the events surrounding the shooting death of an unarmed young black man by a white police officer in Ferguson, Missouri—and a string of similar incidents across the country—have led to increases in crime in the United States. This study tested for the "Ferguson Effect" on crime rates in large U.S. cities.

Methods: Aggregate and disaggregate monthly Part I criminal offense data were gathered 12 months before and after August 2014 from police department data requests and websites in 81 large U.S. cities. The exogenous shock of Ferguson was examined using a discontinuous growth model to determine if there was a redirection in seasonality-adjusted crime trends in the months following the Ferguson shooting.

Results: No evidence was found to support a systematic post-Ferguson change in overall, violent, and property crime trends; however, the disaggregated analyses revealed that robbery rates, declining before Ferguson, increased in the months after Ferguson. Also, there was much greater variation in crime trends in the post-Ferguson era, and select cities did experience increases in homicide. Overall, any Ferguson Effect is constrained largely to cities with historically high levels of violence, a large composition of black residents, and socioeconomic disadvantages.

Conclusions: The national discourse surrounding the "Ferguson Effect" is long on anecdotes and short on data, leaving criminologists largely on the sidelines of a conversation concerning one of the most prominent contemporary issues in criminal justice. Our findings are largely consistent with longstanding criminological knowledge that changes in crime trends are slow and rarely a product of random shocks.

KEYWORDS: crime trends; Ferguson; policing

[1] The authors wish to acknowledge the dozens of law enforcement agencies who willingly made their data available to us. We are indebted to countless crime analysts, police chiefs and command staff for their efforts on our behalf. We would also like to thank the many colleagues that helped in this data acquisition effort as well.

Crime is one of the most important influences on the quality of life in the United States. Beyond the harm faced by crime victims, crime rates are a key structural feature of communities that are associated with long-term negative consequences for residents and the well-being of cities. These consequences include the stigma of being identified as a "bad neighborhood" or "dangerous city" to outsiders and insiders alike, and the resulting deleterious consequences for the area's economic landscape (Besbris, Faber, Rich, & Sharkey, 2015; Sampson, 2012; Xie & McDowall, 2010). Accordingly, understanding changes in crime rates is important for a wide range of academic disciplines and has direct policy implications for the criminal justice system and other social institutions. Since the early 1990s, the United States has enjoyed the longest sustained decline in crime since the FBI began compiling crime statistics in the early 1930s (Blumstein & Wallman, 2006; Fagan, Zimring, & Kim, 1998). However, since the shooting of Michael Brown, an unarmed black man in Ferguson, MO on August 9, 2014, the subsequent civil unrest, and social media attention to his shooting, there has been speculation that a "Ferguson Effect" has ended the great crime decline (Bialik, 2015; Davey & Smith, 2015; Mac Donald, 2015; Rosenfeld, 2015).

Could the events surrounding Ferguson have changed the trajectory of crime trends in the United States? Such a hypothesis is consistent with three potential explanations. The first of these is de-policing, where negative publicity and public protest regarding police behavior leads officers to withdraw from enforcing the law for fear of criticism and lawsuits. From this perspective, police officers throughout the United States may have become hesitant to be proactive out of concerns for being subjected to negative media scrutiny for racial profiling or the use of excessive force (Wolfe & Nix, 2015; see also: Oliver, 2015). Reduced guardianship and lack of enforcement—if widespread enough—may lead to increases in crime rates (Braga, Papachristos, & Hureau, 2014; Levitt, 2002; Marvell & Moody, 1996; Rosenfeld, Deckard, & Blackburn, 2014). The empirical evidence for de-policing is mixed (Shi, 2008; Stone, Foglesong, & Cole, 2009).[2] For example, Shi (2008) showed that in the wake of a highly publicized incident involving a white police officer shooting an unarmed African-American teenager in Cincinnati, OH and subsequent Department of Justice investigation, arrests fell substantially (i.e., evidence of de-policing). However, research showed that an LAPD consent decree did not result in any form of de-policing (Stone et al., 2009). In fact, pedestrian and motor vehicle stops doubled after the consent decree and a higher proportion of such stops resulted in an arrest. Total arrests were also shown to increase post-consent decree. It must be noted, however, that de-policing is difficult to measure and requires measurements of police activity which are generally not publicly available.

Unlike the events spurring the studies of de-policing in Cincinnati and Los Angeles, the shooting in Ferguson occurred in the era of social media. The massive social media response following the events in Ferguson may have precipitated de-policing through contagion, the viral

[2] It is important to note that Kane (2005) demonstrated that under policing was not associated with crime rates in New York City in structurally disadvantaged communities. However, aggressive styles of policing—"over policing"—were shown to be associated with increases in crime over time.

spread of information across social media. Indeed, the search term “Ferguson shooting” yields around 30 million hits on Google, and has led to speculation about such a response by law enforcement officials including several municipal Chiefs and the Director of the FBI. The juxtaposition of concerns about de-policing, the resulting unrest following the initial shooting, and the decision not to pursue criminal charges against the officer coupled with extensive social media coverage of issues in Ferguson make this a compelling issue to examine empirically.

Second, high-profile incidents such as Ferguson may convey to the public that justice is being administered unfairly and lead to challenges to the legitimacy of the law. One response to the belief that the law is not administered fairly is increased participation in crime (Jackson et al., 2012; Tyler, 2006). It is possible that the killing of unarmed citizens by officers sends the signal to some citizens that law enforcement’s values and behaviors are inconsistent with their expectations about how the law should be administered, therefore reducing stakes in conformity, and leading to crime and disorder. Public trust in the police can be precarious, and such shootings may upend that balance, particularly for the most disadvantaged members of American society (Kane, 2005). Public criticism of the police on social media has spread the potential impact of such an effect well beyond the geographic bounds of the St. Louis metropolitan area, often in response to other officer-involved shootings. For example, the death-in-custody of Freddie Gray led to violent rioting in Baltimore and the shooting of John Crawford III in Beavercreek, OH resulted in a Department of Justice investigation. Public outcry regarding the shooting of Laquan McDonald in Chicago has led to calls for the resignation of Mayor Rahm Emanuel. In short, it is clear that citizens in a number of U.S. cities are calling into question the legitimacy of police use of force.

The third explanation is that crime declines had reached their nadir and any increases in crime were due to factors unrelated to Ferguson. After all, the large declines in crime observed over the past two decades likely will eventually level out or even increase at some point. Such a view is consistent with several threats to internal validity, including history and regression to the mean (Shadish, Cook, & Campbell, 2002). This argument finds the relationship between a Ferguson Effect and increased crime rates attributed to extraneous factors and thus spurious. This is a plausible argument, given the length and magnitude of the crime decline which may not be sustainable. Nonetheless, whether this nadir occurred coincidentally at the same time as Ferguson would raise serious questions about turning points in crime trends.

Even so, should the Ferguson Effect be observed systematically *throughout* the United States or are claims of de-policing and challenges to the legitimacy of the police idiosyncratic to particular cities? The heterogeneity among large U.S. cities makes it likely that exogenous factors will be experienced differently. Social media has played an important role in drawing attention to Ferguson and related events (Wolfe & Nix, 2015), making it possible for events to be observed in one city and their impact to be felt in others. In less than two weeks after the shooting, the *Wall Street Journal* reported over 7.8 million tweets using the #Ferguson hashtag, and the *New York Times* reported considerable misinformation on social media about the shooting as well as the ensuing social unrest (Bilton, 2014; Zak, 2014). The effects of social media may lead to de-policing or erosion in the legitimacy of the law, which in turn could lead to increases in crime across the United States.

Importantly, the Ferguson Effect has been blamed for apparent increases in violent crime in several U.S. cities by government leaders, law enforcement executives, and academics alike. FBI Director James Comey even recently suggested that the Ferguson Effect has led to increases in violent crime in some cities by stating, “I don’t know whether that explains it entirely, but I do have a strong sense that some part of the explanation is a chill wind that has blown through American law enforcement over the last year” (Schmidt & Apuzzo, 2015). Ironically, Director Comey heads the agency responsible for producing the Uniform Crime Reports in the United States and he himself lacked the full data on crime to draw the conclusions he announced. Indeed, there is little empirical evidence and lots of speculation about this question. A brief paper examined St. Louis crime rates over time and found very limited support for such an effect (Rosenfeld, 2015). Another analysis focused on year-by-year differences in homicide across 60 cities and found a 16 percent increase in 2015 from 2014 (Bialik, 2015). A recently released *Brennan Center for Justice* report examined homicide changes between 2014 and 2015 in 25 of the 30 largest U.S. cities, finding that homicide rates increased by 15 percent (Friedman, Fortier, Cullen, & James, 2015). While these studies reveal important insight into crime in the United States, they are either narrow due to the number of cities or the number of crime types included in their analyses. These studies lack the broad theoretical rationale presented here to anticipate that a Ferguson Effect may extend to a broader cross-section of our largest cities and to forms of crime other than homicide.

This study examines whether crime trends changed systematically after the Ferguson shooting and if there were idiosyncratic changes across U.S. cities. We conduct our analysis among 81 U.S. cities with populations exceeding 200,000 persons using discontinuous growth modeling that incorporates between-city variability in FBI Part I aggregate and disaggregate crime trends before and after Ferguson. After all, there is considerable heterogeneity across U.S. cities. Even among large cities, there is variation in region, crime level, policing style, and the size of the police force, among other things which could either buffer or bolster any “Ferguson Effect” on crime. It is important to note that the city-level crime data used in this analysis cannot establish whether loss of legitimacy or de-policing is at the root of an observed increase in crime, or whether contagion induced by social media was responsible for transmitting these changes. Rather, our central goal is to provide the most comprehensive investigation into the Ferguson Effect on crime trends in the United States. If there is a Ferguson Effect, it could challenge the decades-long decline in serious crime, particularly homicide. But of course, without data, these arguments remain speculative. FBI Director Comey echoed this sentiment by suggesting that “Data is a dry word, but we need better data” concerning the Ferguson Effect. We agree—this issue is important because considerable public and private resources are committed to responding to crime and their use should be guided by data and not speculation. Data and analysis must serve as the foundation of evidence-based decision making and policy development.

METHODS

Data

This study examined official crime data in U.S. cities. Monthly crime data were collected from police department data requests and police department websites in 81 of the 105 U.S. cities with populations exceeding 200,000 persons in 2010. Larger cities were the focus of this study because crime recording practices are more reliable, and the volume of criminal activity is greater and less subject to random fluctuation in the numerator, than in smaller cities (Maltz, 2006). Using 200,000 persons as our target population threshold also provided the twofold benefit of increasing generalizability to a larger group of cities than prior studies (Bialik, 2015; Friedman et al., 2015; Rosenfeld, 2015), which works to temper concerns about analyses being underpowered to detect statistical differences.[3]

We focused on the seven Part I Uniform Crime Report (UCR) offenses that include measures of violent crime (criminal homicide, forcible rape, robbery, and aggravated assault) and measures of property crime (burglary, larceny-theft, and motor vehicle theft). Included in the UCR are crimes reported to the police and the hierarchy rule applies to UCR offenses. Only the most serious of multiple-offense incidents is recorded. Detailed information about the UCR reporting can be found in the summary reporting system manual (Federal Bureau of Investigation, 2013).

Crime was standardized by city population as monthly crimes per 100,000 citizens. Twelve months of pre- and post-Ferguson data were included. This provides us with a full year of crime data before and after Ferguson, allowing us to determine if crime trends were indeed redirected in the months following Ferguson. There were two reasons for inducing symmetry in our research design. First, our data were temporally censored in the period after Ferguson. We collected and analyzed the data between October and December 2015, which made September the last available month, permitting a 12-month post-Ferguson period of observation. Second, a 12-month pre-Ferguson period was long enough to assess crime trends, but short enough to protect the findings from threats to internal validity. Indeed, a longer pre-Ferguson window of study would have made our findings more susceptible to historical, maturational, and statistical regression factors. Future research might consider alternative research designs, namely, longer periods of observation, especially as additional data become available.

Complete or partial crime data were included for 81 of the 105 largest U.S. cities. Jurisdictions were excluded if they did not maintain at least four months of post-Ferguson crime data. Homicide was the modal crime type included (N=81 cities), while data on rape was

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[3] FBI thresholds are populations of 250,000 or more for Group I cities (76 total), while populations between 100,000 and 249,999 for Group II cities (209 total). There is no bright line that guides population thresholds in macro-level criminological research. In addition to the reasoning described above (measurement, generalizability, power), our selection strategy also aimed to maximize crime, demographic, economic, political, and social variation in the cities represented in this study and operated under the practical restraint that the authors collected data city-by-city, negotiating the unique policies and practices of data collection in 81 cities. While we are grateful for the support of the dozens of police chiefs, crime analysts, and command staff in this effort, it would have been unmanageable to extend this data collection to FBI Group II cities, or even smaller cities like Ferguson, MO (population 21,111).

available for 76 cities. Jurisdictions without full data on total crime (*N*=5), violent crime (*N*=5), and property crime (*N*=2) were excluded from aggregate analyses. An average of 11 months of post-Ferguson data were available across the crime types for the 81 cities. Overall, our data include 87 percent—18,321 of the 20,250 possible data points—of the data available across the 10 crime types analyzed in 81 cities over a 25-month period. Appendix A includes a list of the cities that were included in the study.

All 2013 monthly data were obtained from the UCR, while we retrieved 2014 and 2015 data either from police department websites (*N*=34 cities) or agency contacts (*N*=47 cities).[4] There were no statistically significant differences in pooled pre- or post-Ferguson crime rates between the sample cities based on the approach used to obtain the data, nor did any of the empirical Bayes predictions differ statistically at the 0.05 level. However, across the seven crime types, data obtained from department websites produced fewer post-Ferguson valid points (mean=71 months) compared to requested data (mean=77 months). This difference is largely attributed to the fact that more recent information was available from data received in response to requests to departments. On average, we had about an additional month of data across all seven crime types; however, the total number of valid data points did not differ statistically. Overall, this means that the potential bias introduced by our data collection strategy is exceptionally low.

There were no statistically significant differences in the pooled pre-Ferguson aggregate crime rates (total, property, and violent) between the excluded and included cities. However, when disaggregated by crime type, homicide and aggravated assault in excluded cities were about 65 percent the size of included cities. All of the structural characteristics of cities were statistically equivalent between included and excluded cities. The cities included in the study, nonetheless, provide a good representation of large U.S. cities. Moreover, our analyses focus on changes in crime trends *within*-cities rather than *between*-cities, which helps minimize the significance of the difference between excluded and included cities.

As we note below, there are many challenges when gathering data to address contemporary issues related to crime, such as the Ferguson Effect. This article represents a systematic attempt to bring comprehensive evidence to bear on a topic that has commanded the attention of public officials at the highest levels, mainstream national news media, and the general public on social media outlets. This article addresses a topic of prominent national discourse that is long on anecdotes and speculation and short on data.

Analytic Strategy

A discontinuous growth model was used to assess the Ferguson Effect on within-city changes in crime trends (Singer & Willett, 2003). A multilevel model was constructed to analyze the research questions and our primary level 1 model takes the following form:

$$Y_{ti} = \pi_{0i} + \pi_{1i}TREND_t + \pi_{2i}POSTTREND_t + \epsilon_{ti}$$

[4] Even at the time of this writing, January 2016, the 2014 UCR data were still not deposited on ICPSR, therefore it remained necessary for us to rely on our data collection strategy.

where *Y* represents the crime rate at a given month *t* for city *i*, π_{0i} is the crime rate for city *i* in August 2014, π_{1i} is a linear trend for city *i* where *TREND* is incremented by 1 for each month and is centered at August 2014, and $\pi_{2i}$is a linear trend for city *i* where *POSTTREND* is incremented by 1 for each month succeeding August 2014. Errors, ϵ_{ti}, are assumed to be independent and normally distributed; residual diagnostics confirmed this. Some outliers (±4 standard deviations) led us to use robust standards to relax the assumption of correct specification.

This study treats the events surrounding Ferguson as an exogenous shock that cities experienced. Consistent with discontinuous growth models, the two linear trends, π_{1i} and π_{2i}, are interpreted additively. In the months leading up to Ferguson, the crime trend for city *i* is represented by π_{1i} because *POSTTREND* is fixed to 0. In the months following Ferguson, the crime trend is represented by π_{1i} + π_{2i}, where coefficients for π_{2i} indistinguishable from 0 indicate no change in the crime trend post-Ferguson, while coefficients above or below 0 represent a redirection in crime trends occurring in the months succeeding Ferguson. All of the models are adjusted for seasonality, a well-known correlate of crime rates relevant to the study of crime trends (Baumer & Wright, 1996; Rosenfeld, 2015), by introducing fixed effects for the month using August as the reference category.

Random effects were introduced to allow the intercept and slopes to vary across cities, with our level 2 models represented as follows:

$$\pi_{0i} = \gamma_{00} + \zeta_{0i}$$
$$\pi_{1i} = \gamma_{10} + \zeta_{1i}$$
$$\pi_{2i} = \gamma_{10} + \zeta_{2i}$$

where γ_{00} represents the mean crime rate the month of the Michael Brown shooting (August 2014), γ_{10} represents the mean crime trend prior to Ferguson, and γ_{20} represents the mean change in post-Ferguson crime trends. These coefficients are represented as the fixed effects in Tables 1 and 2. By introducing random effects, it permits us to not only investigate city-level variation in crime rates at the time of Ferguson, but also variation in the trends before and after Ferguson. This allows us to detect if there were truly changes in crimes trends within-cities, as each city maintains its own individual intercept and trajectories. All analyses were conducted using the *me* suite in Stata 14.0 (StataCorp, College Station, TX).

Results

Table 1 shows that the total crime rate was decreasing in the 12 months prior to Ferguson [*b*=-1.43, *P*<0.05], consistent with a long-term trend of declining crime rates in the U.S. (Xie, 2014; Zimring, 2006). The overall pre-Ferguson crime decline was driven by trends in property crime [*b*=-1.52, *P*<.01] but not violent crime [*b*=0.03, *P*=0.84]. Whereas the violent crime rate trend was essentially flat in the 12 months before Ferguson, property crime rates were decreasing by over 1.6 crimes per capita each month.

Table 1. Unstandardized Coefficients from Discontinuous Growth Models of Trends in Total, Violent, and Property Crime Rates.

Fixed effects	Total Crime Rate		Violent Crime Rate		Property Crime Rate	
Constant	452.06	(18.10)**	73.43	(5.13)**	376.27	(14.64)**
Crime trend	-1.43	(0.65)*	0.03	$(0.17)^{ns}$	-1.52	(0.59)**
Post-Ferguson trend	1.00	$(1.17)^{ns}$	0.34	$(0.22)^{ns}$	0.72	$(1.05)^{ns}$
Random effects						
Constant ζ_{0i}	19582.20		1445.55		14099.80	
Crime trend ζ_{1i}	25.98		1.77		23.00	
Post-Ferguson trend ζ_{2i}	79.114		2.17		69.60	
Sample size						
N of cities	76		76		79	
N of cities*months	1764		1769		1845	

NOTES: Robust standard errors are given in parentheses. Random effects report between-city variance. All results are seasonality-adjusted.
ns, *P*>0.10; +, *P*<.10; *, *P*<0.05; **, *P*<.01

After the shooting of Michael Brown, and the subsequent social unrest and social media responses, was there a *systematic* change in crime trends in large U.S. cities? We find no evidence in support of this contention. Our results reveal that the post-Ferguson trends in total [*b*=1.00, *P*=0.39], violent [*b*=0.34, *P*=0.12], and property [*b*=0.72, *P*=0.50] crime were statistically insignificant. While the post-Ferguson trend coefficients for all three aggregate crime were positive, they were not large enough to be statistically distinguishable from the pre-Ferguson crime trend, which was flat for violent crime and declining for property crime. Altogether, we can conclude that there is no systematic evidence of a Ferguson Effect on aggregate crime rates throughout the large U.S. cities represented in this study.

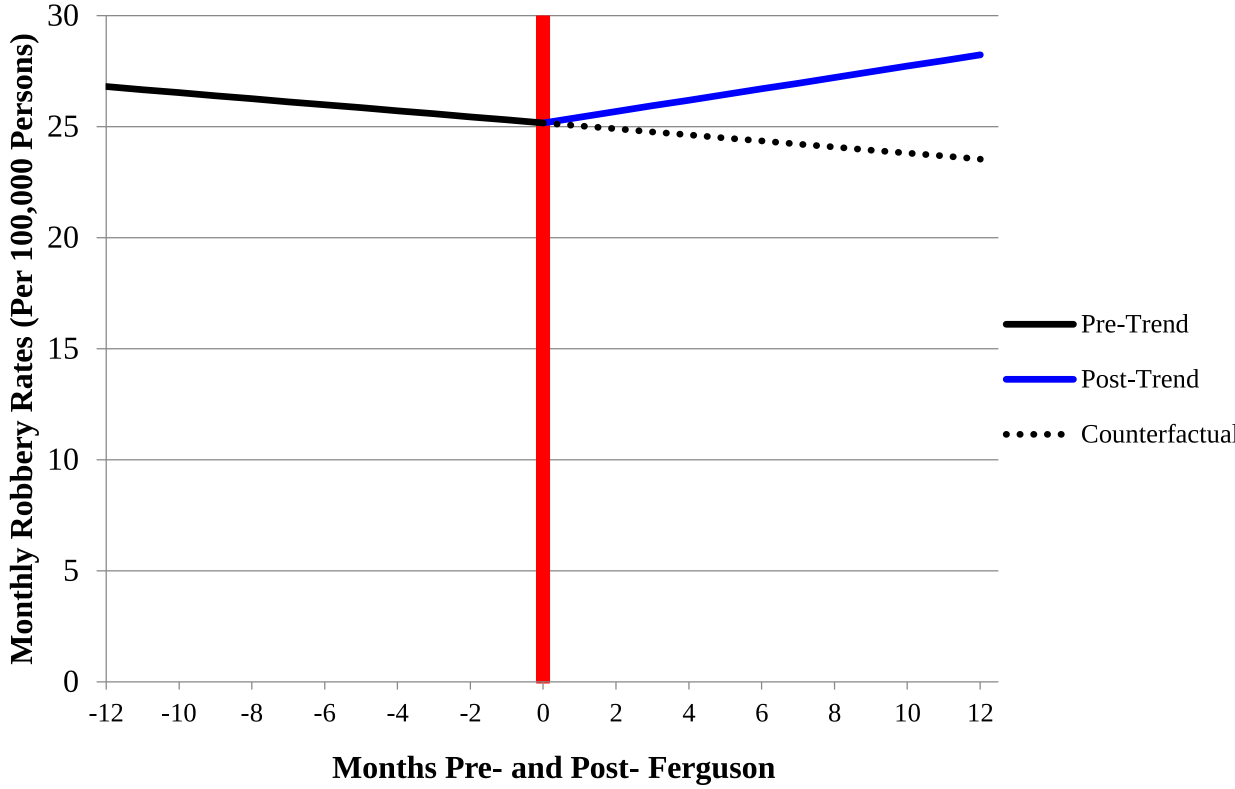
Table 2 disaggregates the total crime rate by Part I offenses, as aggregate crime rates might mask important trends within specific offense types and there is a theoretical rationale motivating such analyses. In the 12 months leading up to Ferguson, monthly crime trends were flat for homicide, aggravated assault, larceny, and motor vehicle theft, increasing for rape [*b*=0.06, *P*<.01], and decreasing for robbery [*b*=-0.13, *P*<.10] and burglary [*b*=-0.50, *P*<.01]. For the post-Ferguson trends, the only crime type that resembles the expected changes associated with a Ferguson Effect was robbery, which increased at a monthly rate of 0.26 incidents per capita [*P*<.05]. Given that our post-trends are estimated additively with the pre-trend, this means that robbery rates were falling by 0.13 per capita monthly, only to shift in the opposite direction resulting in an increase of 0.12 robberies per capita monthly [-0.132+0.255]. We provide a graphical illustration of this redirection in Figure 1. All of the remaining outcomes revealed post-Ferguson crime trends that were statistically indistinguishable from pre-Ferguson crime trends. While there was a clear absence of evidence for a Ferguson Effect among aggregated crime

Table 2. Unstandardized Coefficients from Discontinuous Growth Models of Crime Trends Disaggregated by Crime Type.

Fixed effects	Homicide Rate	Rape Rate	Robbery Rate	Aggravated Assault Rate	Burglary Rate	Larceny Rate	Motor Vehicle Theft Rate
Constant	0.944 (0.109)**	4.85 (0.30)**	25.17 (2.01)**	41.86 (3.35)**	82.94 (4.36)**	248.04 (10.70)**	45.29 (3.43)**
Crime trend	-0.003 (0.005)^{ns}	0.06 (0.02)**	-0.13 (0.07)+	0.09 (0.13)^{ns}	-0.50 (0.18)**	-0.88 (0.56)^{ns}	-0.12 (0.11)^{ns}
Post-Ferguson trend	0.015 (0.009)^{ns}	-0.04 (0.03) ^{ns}	0.26 (0.12)*	0.13 (0.14)^{ns}	0.19 (0.23)^{ns}	0.26 (0.89)^{ns}	0.23 (0.23)^{ns}
Random effects							
Constant ζ_{0i}	0.6200	5.358	216.77	696.69	1275.14	7746.04	685.50
Crime trend ζ_{1i}	0.0005	0.004	0.35	1.15	2.07	23.29	0.83
Post-Ferguson trend ζ_{2i}	0.0030	0.012	0.77	0.96	2.14	52.45	3.05
Sample size							
N of cities	81	76	79	80	79	79	79
N of cities*months	1902	1781	1850	1865	1850	1845	1850

NOTES: Robust standard errors are given in parentheses. Random effects report between-city variance. All results are seasonality-adjusted.
ns, *P*>0.10; +, *P*<.10; *, *P*<0.05; **, *P*<.01

Figure 1. Predicted Values of the Pre- and Post- Ferguson Trends in Monthly Robbery Rates Per 100,000 Persons (N=79).



types, the conclusion from this disaggregated analysis is that changes in robbery rates constitute the lone exception to a spurious Ferguson Effect.[5]

Nonetheless, the post-Ferguson crime trends were not universally equivalent to pre-Ferguson crime trends across large U.S. cities. Tables 1 and 2 report the variance in the pre- and post-Ferguson trends for all 10 outcomes examined in this article. Overall, the variance in the post-Ferguson trend was much greater than the pre-Ferguson trend. Indeed, the variance in total crime rates was 3 times as large after Ferguson than before; for homicide rates, the variance was 5.7 times greater. This means that crime trends in the post-Ferguson era varied considerably

[5] For those concerned with multiple testing we used the Benjamini-Hochberg (1995) procedure to compute critical values for rejecting the null hypothesis, as follows: $p_k<(i/m)*Q$, where p is the p value for variable k, i is the rank of the p value in ascending order for k variables, m is the total number of tests, and Q is the false discovery rate. For robbery rates, p=0.027, i=1 (smallest p value), m=4 (m of violent crimes), and Q=0.10 (false discovery rate of 10%), the Benjamini-Hochberg critical value of 0.025 exceeded that of the p value from the mixed effect models. Adjustments for multiple testing, particularly the Bonferroni correction which is somewhat commonly seen in criminological outlets, are highly criticized because there are few established rules for correction. The criterion for deciding the family wide error rate (FWER) is vague and what constitutes simultaneous inference is unclear (Cabin & Mitchell, 2000). And, as Cabin and Mitchell pointed out, is the correction table- or manuscript-wide, or even the career of a dataset? It is also subject to misuse. Indeed, using Nakagawa's (2004) hypotheticals, varying the number of outcomes could push the p value above or below the critical value. While the purpose of these corrections is to reduce Type I error, it could ultimately introduce Type II error. We view this as a net zero gain, and thus urge future research examining issues comparable to ours to pay close attention to robbery rates.

across cities, with some cities experiencing large changes in their crime trends, while others experienced little to no change. It is important to emphasize that such variation across cities does not alter the overarching conclusion that evidence of an overall Ferguson Effect on crime rates in large U.S. cities is negligible. But it does mean that there is less stability in crime trends in the post-Ferguson era, which could point to heterogeneous responses to the exogenous shock of Ferguson.

Figure 2 plots the empirical Bayes predictions for the post-Ferguson monthly rate of change in homicide per capita for 81 cities with valid data on homicide. We examined homicide because it is the most reliably recorded of the seven Part I crimes and a barometer of the overall crime trend in cities. Notably, St. Louis, the metropolitan area that includes Ferguson, scored among cities with the largest increases in homicide rate trends. To the extent that any Ferguson Effect exists, it appears to be constrained to a small number of cities, particularly cities with historically high homicide rates, as we show below.

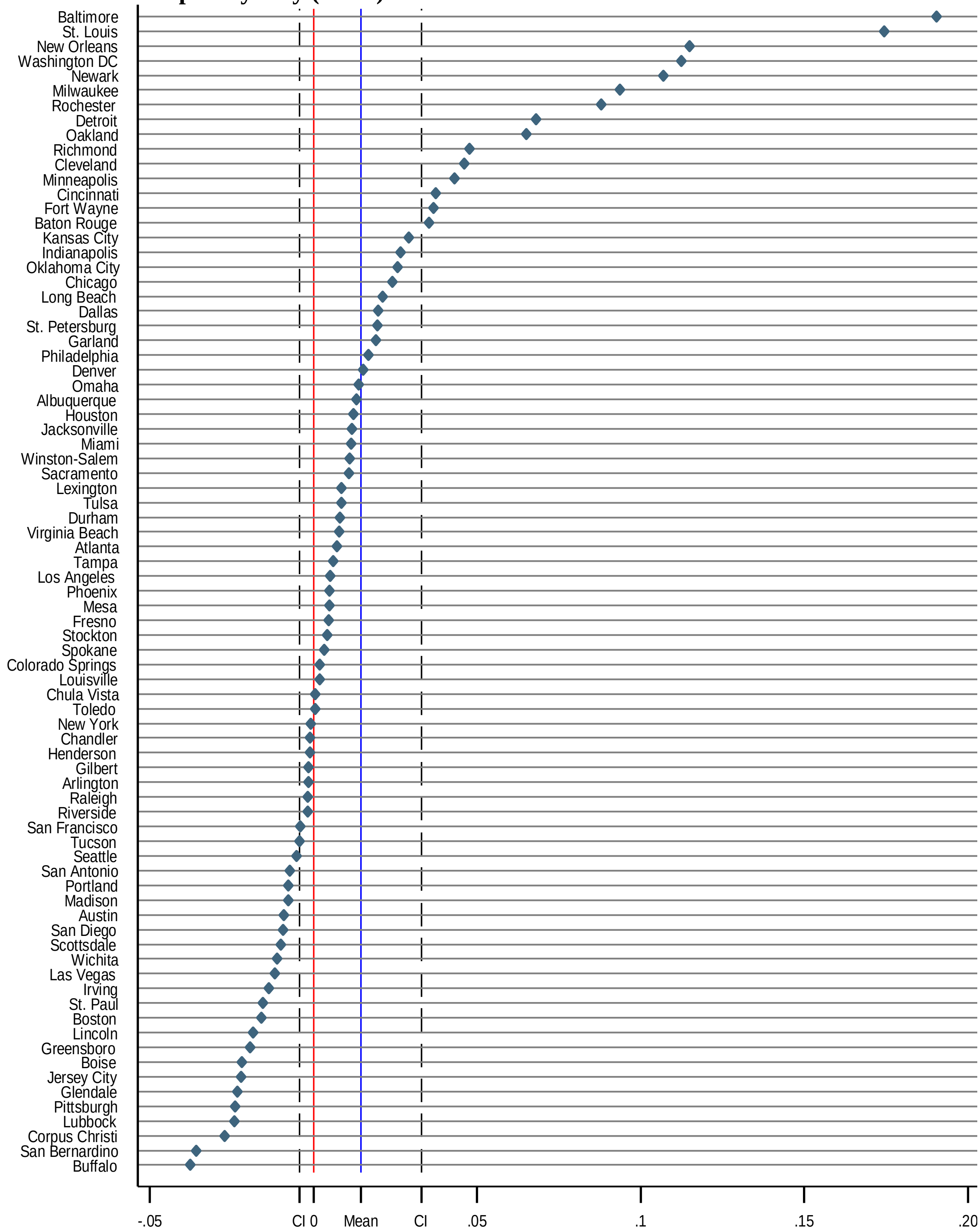
Table 3 examines the characteristics of cities based on the magnitude of changes to homicide trends in the post-Ferguson era. Cities were divided into three categories based on empirical Bayes predictions of post-Ferguson homicide trends: negative rate of change (N=27), a low, yet positive, rate of change (N=27), and a high and positive rate of change (N=27). In the latter group, assuming the rate of change remains stable, it would take about two years for cities to witness an entire one-unit increase in the homicide rate. We compare these three groupings of cities in order to determine if there were factors associated with heterogeneity in post-Ferguson homicide trends, and our findings reveal that there were indeed important differences.

As the rate of change in homicide trends becomes positive and larger in magnitude, cities tended to have higher pre- and post-Ferguson pooled violent crime rates, a higher rate of police officers per 1,000 citizens, a greater composition of black residents and a smaller composition of white residents, and greater socioeconomic disadvantages that are typically associated with high homicide rates (Pratt & Cullen, 2005). The differences are especially pronounced between the high/positive change cities compared to the negative and low/positive change cities. Indeed, the cities with a flat or negative homicide rate trend share a great deal of similarities in their demographic, social, economic, and criminal profiles. Moreover, heterogeneity in homicide rate increases is not related to population size. Overall, these increases are confined to a smaller group of cities; however, these cities are very different from those which experienced little or no changes in homicide rate trends after the events that propelled Ferguson to the center of national and international scrutiny surrounding criminal justice system policies and practices in the United States.

Discussion

Declining crime rates since the 1990s have marked one of the most significant “civilizing” trends in U.S. history (Pinker, 2011). Large, sustained declines in crime have made the United States much safer. The sources of the crime decline throughout the 1990s and 2000s are complex, ranging from improvements in the economy and the decline of the crime prone

Figure 2. Empirical Bayes Predictions of the Post-Ferguson Rate of Monthly Change in Homicide Per Capita by City (N=81)



Note: CI=confidence interval; Mean=post-Ferguson trend unstandardized coefficient

Table 3. Differences Across Cities by Monthly Changes in Post-Ferguson Homicide Rate Trends.

	Homicide Rate Trend			
	Negative Rate of Change	Low/Positive Rate of Change	High/Positive Rate of Change	
	Mean/%	Mean/%	Mean/%	*P*
Empirical Bayes Predictions				
Pre-Ferguson	0.0058	-0.0015	-0.0130	
Post-Ferguson	-0.0150	0.0046	0.0557	
Pre-Ferguson Pooled Crime Rates				
Homicide Rate	0.59	0.65	1.54	0.000
Total Crime Rate	400.71	376.92	470.10	0.043
Violent Crime Rate	51.97	49.50	89.95	0.000
Property Crime Rate	348.74	327.47	382.34	0.234
Post-Ferguson Pooled Crime Rates				
Homicide Rate	0.56	0.66	1.72	0.000
Total Crime Rate	388.11	380.32	458.42	0.082
Violent Crime Rate	56.64	52.45	91.08	0.001
Property Crime Rate	331.47	322.51	364.86	0.367
Structural Characteristics				
Population	461,979	888,547	577,967	0.283
Officers Per Capita	2.08	2.06	3.08	0.000
% Consent Decree	7.4%	3.7%	14.8%	0.335
% Minority Mayor	11.1%	22.2%	37.0%	0.079
% White	64.8%	61.4%	48.6%	0.000
% Black	11.6%	18.0%	35.5%	0.000
% Hispanic	25.4%	23.9%	17.0%	0.153
% Female HH w/Children	7.8%	8.7%	10.4%	0.000
% Unemployed	11.5%	12.3%	13.9%	0.047

NOTES: Means and prevalences are reported. One-way ANOVA and chi-square tests are used to determine statistical significance. Cities were grouped into three post-Ferguson homicide trends: cities with a negative rate of change N=27, cities with a low/positive rate of change N=27, and cities with a high/positive rate of change N=27.

segment of the population (ages 15-29 years) to the decline of crack cocaine markets and increases in imprisonment (Blumstein & Wallman, 2006; Levitt, 2004; National Research Council, 2008). Of course, policing is another important explanation. Any redirection in such a sustained decline in crime is invariably a cause for concern.

In recent years, the public, the police and elected officials are increasingly subject to the social contagion effects of social media. The killing of Michael Brown in Ferguson, MO appears to have sparked one such social process, reinforced by repetitive mentions of the Ferguson Effect in social and other media by political and law enforcement leaders. Despite widespread coverage of protests and the responses to officer-involved shootings by social and other media and speculation concerning the Ferguson Effect, the incident was not a sentinel event that changed the direction of the crime decline. Our analysis was well positioned to identify the existence of a Ferguson Effect on crime rates among the largest sample of U.S. cities examined to date. We found that the crime decline has not changed substantially in large American cities in the 12 months after Ferguson. Simply put, we observed no systematic and widespread change in crime trends among the cities in our study beyond robbery rates. Our analysis thus confirms the long-held understanding that the causes of crime reflect slow processes and are not amenable to sudden shocks (LaFree, 1999; National Research Council, 2008). With this in mind, several issues require further discussion.

First, although the overall null Ferguson Effect was robust for both total and disaggregated crime scales, it is important to note that our analysis showed that robbery rates significantly increased in the study cities post-Ferguson. This finding suggests that a Ferguson Effect may have occurred but its influence is limited to robbery rates. Although our data cannot offer insight into the causal mechanism behind this observation, it appears that robbery rate increases began about the same time as Michael Brown's death. We urge future research examining issues comparable to ours to pay close attention to robbery rates.

Next, while it would be a rush to judgement to suggest that the Ferguson Effect will end the decades-long crime decline, we observed substantial variation in crime trends in the post-Ferguson era. Several cities (Baltimore, St. Louis, Newark, New Orleans, Washington, D.C., Milwaukee, and Rochester among others) experienced large increases in homicide rates following the events in Ferguson. Accordingly, the data offer preliminary support for a Ferguson Effect on homicide rates in a few select cities in the United States. What is important about these cities is that they had much higher crime rates before Ferguson, which in turn may have primed them for increases in crime. Cities with post-Ferguson increases in crime tended to have a higher proportion of black residents, lower socioeconomic status, and more police per capita—important macro-level correlates of crime rates (Pratt & Cullen, 2005; Sampson, 2012). Simply put, these other predictors of crime rates lead to questions that may inhibit any ability to attribute crime increases specifically to the Ferguson Effect in these cities, and require further—and more formal—moderator analyses to isolate the constellation of factors that such cities share in common.

What our analysis cannot speak to is the extent to which de-policing or a crisis in police legitimacy have occurred post-Ferguson, and if so, the impact it may have had on crime rates. Indeed, it is not possible to use these data to discriminate between such hypotheses or to establish the role, if any, that protests may have played in contributing to de-policing by some officers or de-legitimizing the police in the eyes of many citizens. Anecdotal evidence concerning de-policing abounds in social media, a medium that has provided a front-row seat to the civil unrest after a number of police killings of citizens (Wolfe & Nix, 2015). What we do know, however, is that if de-policing or a legitimacy crisis are occurring, neither is impacting crime rates systematically across large U.S. cities. Subsequent analyses should examine these propositions as they are critical to the effective administration of justice in the United States. Given the complexity of such analyses, it may be beneficial to focus efforts on the apparent systematic robbery rate increase and the homicide rate increases observed in the handful of violent, racially diverse cities discussed earlier.

This study is not the final word on the Ferguson Effect. There are alternative ways to test for a Ferguson Effect—we chose one method that addresses a key question in this debate: did the events in Ferguson lead to a redirection in crime trends? While our study provides important answers to this question there are several issues that represent opportunities for future research. The first is the nature of our sample. We chose to use 200,000 as the population cutoff for inclusion in the sample. While the 81 cities in our sample account for both a large portion of the U.S. population (17 percent) and violent crime (29 percent) in 2014, it omits smaller cities such as Ferguson. There may be important differences between the large cities in our sample and medium and small cities that our analysis does not capture. As such, our results are only generalizable to those cities included in our sample. This analysis cannot speak to whether a Ferguson Effect has or has not occurred in smaller towns throughout the U.S. Future research is needed to answer this question.

Second, outside of seasonality-adjustments, our models excluded both time-varying confounders and mediators. As we have mentioned above, securing crime data alone was a time-intensive task; that burden would have increased considerably or impossibly if we attempted to secure data that would allow us to simultaneously model our mediating mechanisms (e.g., depolicing, legitimacy, social media). Therefore, while our results point to a null hypothesis, there could be factors suppressing the Ferguson Effect.

Third, we use a 12-month pre- and post-Ferguson series of crime data. More months may have made the estimates more stable, perhaps increasing the chance of finding statistically significant changes in crime trends. However, a shorter window of observation does provide advantages that limit threats to internal validity, namely, history, maturation, and regression to the mean. Fourth, despite repeated efforts we were unable to obtain all of the crime data for cities that met our sampling criteria (over 200,000 population). While three-fourths of all large U.S. cities are represented in our study, including 10 of the largest U.S. cities, some bias in our findings may remain because of the cities for which data were unavailable.

Before concluding, there is another important issue regarding data worthy of note. In the wake of concerns about an abrupt change in the decades-long crime decline, the data to assess this question were not readily available to policy makers, the public, or researchers. Crime counts were collected from police department websites and through direct requests for crime data made to police departments. Indeed, this was an arduous and time-consuming process. As others have noted, important issues of public policy such as crime should be addressed with current and publicly accessible data (Rosenfeld, 2007). Data should be readily available for addressing questions such as what causes crime rates to go up or down given the potentially far-reaching public health and public safety policy concerns associated with such investigations. Reliable and publicly available crime data is necessary to ground policy decisions on evidence-based, scientific knowledge.

In conclusion, tragic events such as those in Ferguson, Staten Island, Baltimore, and North Charleston, to name a few, have sparked debate over important issues such as police-community relations and police legitimacy. Such debates, however, should be informed by solid data and careful analysis. Policy decisions that are not evidence-based can negatively impact public safety, curtail debate and action on important issues such as mass incarceration, or, at the very least, result in ill-advised expenditure of tax dollars. We sought to bring empirical evidence to the Ferguson Effect debate. On the whole, there is no nationwide Ferguson Effect on crime rates. At the same time, we did observe an increase in robbery rates in the United States and homicide rates in several cities that began at the same time as Ferguson. Our hope is that these results will help provide evidence-based discussions of the Ferguson Effect, specifically, and changes in crime trends more generally.

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Appendix A. Source of 2014 and 2015 Data

Agency Websites/Open Data (N=48)	Agency Analysts/Contacts (N=33)	Missing Cities (N=24)
Arlington, TX	Albuquerque, NM	Anaheim, CA
Atlanta, GA	Baltimore, MD	Anchorage, AK
Austin, TX	Boise, ID	Aurora, CO
Baton Rouge, LA	Boston, MA	Bakersfield, CA
Chula Vista, CA	Buffalo, NY	Birmingham, AL
Dallas TX	Chandler, AZ	Charlotte, NC
Fort Wayne, IN	Chicago, IL	Chesapeake, VA
Garland, TX	Cincinnati, OH	Columbus, OH
Houston, TX	Cleveland, OH	Des Moines, IA
Irving, TX	Colorado Springs, CO	El Paso, TX
Jacksonville, FL	Corpus Christi, TX	Fort Worth, TX
Jersey City, NJ	Denver, CO	Fremont, CA
Lincoln, NE	Detroit, MI	Hialeah, FL
Long Beach, CA	Durham, NC	Honolulu, HI
Mesa, AZ	Fresno, CA	Irvine, CA
Minneapolis, MN	Gilbert, AZ	Laredo, TX
Oakland, CA	Glendale, AZ	Memphis, TN
Oklahoma City, OK	Greensboro, NC	Mesa, AZ
Omaha, NE	Henderson, NV	Modesto, CA
Philadelphia, PA	Indianapolis, IN	Nashville, TN
Portland, OR	Kansas City, MO	Norfolk, VA
Richmond, VA	Las Vegas, NV	North Las Vegas, NV
Riverside, CA	Lexington, KY	Orlando, FL
San Antonio, TX	Los Angeles, CA	Plano, TX
San Bernardino, CA	Louisville, KY	Santa Ana, CA
San Diego, CA	Lubbock, TX	
San Jose, CA	Madison, WI	
St. Louis, MO	Miami, FL	
St. Paul, MN	Milwaukee, WI	
St. Petersburg, FL	New Orleans, LA	
Tulsa, OK	New York, NY	
Virginia Beach, VA	Newark, NJ	
Washington, D.C.	Phoenix, AZ	
	Pittsburgh, PA	
	Raleigh, NC	
	Reno, NV	
	Rochester, NY	
	Sacramento, CA	
	San Francisco, CA	
	Scottsdale, AZ	
	Seattle, WA	
	Spokane, WA	
	Stockton, CA	
	Tampa, FL	
	Toledo, OH	
	Tucson, AZ	
	Wichita, KS	
	Winston-Salem, NC	