RE: ACLU Matter vs. – REF# 1340012232

Ecological Analysis of Monthly Stop Data July through December 2016 (Period 2): Input to Hon. Arlander Keys' (Ret.)

TECHNICAL REPORT

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Contents

Executive Summary

The aim of this report is twofold. First, it identifies and describes ethnoracial differentials in investigatory stop counts from July through December of 2016. More specifically, analyses described herein examine the extent to which stop counts of non-Hispanic Blacks differ from those of non-Hispanic Whites, and the extent to which stop counts of Hispanic Whites differ from those of non-Hispanic Whites. The ethnoracial stop counts for each group are compared after benchmarking them against different ethnoracial-specific features. All inferential analyses are at the police district level. Second, analyses here cover the second reporting period of the settlement agreement. As such, where possible, this report identifies any notable differences between the findings of the ecological report for the first period and the second period.

A series of descriptive analyses were conducted at the city level. This report uses a total of 51,248 investigatory stop reports for the period July through December 2016. (Authors acknowledge that this number differs from the total number of investigatory stop reports for the period, 51,538, shown in Table 1 of the Post Stop Outcomes report. Moreover, the total number of stops noted in this report excludes 18 stops with missing district information, plus 86 stops that occurred in districts 31 and 41.) Among ethnoracial groups over the six-month study period, the average monthly stop rate for Blacks was 7.23 per 1,000 residents, followed by Hispanics (3.7), then Whites (0.82). Compared to the last six months of 2015, current ethnoracial average monthly stop rates represent an 81% decrease for Blacks, an 82% decrease for Whites, and a 79% decrease for Hispanics. When stop rates were calculated using the previous month's violent arrests, descriptive evidence remained of ethnoracial disparities during select periods. For example, for five out of six months, non-Hispanic Whites demonstrated the lowest stop rates, followed by Hispanics, and then non-Hispanic Blacks.

Negative binomial models permitted statistical inferences about some of the descriptive findings highlighted thus far. These inferences permitted separating differences arising from randomness or noise in the data from more substantial differences. Using such models, we transformed stop counts into rates in three ways: using violent arrests from the month prior for each ethnoracial group, using total arrests from the month prior for each ethnoracial group, and using an age-weighted version of each district's total population. We believe that the models using violent arrests as the benchmark are the most reliable, and provide estimates that align with differentials observed in other recent stop and frisk research. Moreover, we have attempted to address the limitations of using violent arrests as an exposure measure by using spatial Empirical Bayesian smoothing. This shift responds to concerns expressed about the ecological models in the Period 1 ecological report.

Models of stop counts benchmarked against earlier violent arrests showed that stops of non-Hispanic Blacks exceeded those of non-Hispanic Whites by 82% during the second reporting period. That effect was significant while controlling for changes over time, district socioeconomic status, district residential stability, and district racial composition. In other words, at an *areal* level, the rate at which earlier violent arrests produced later investigatory stops proved higher when the *group* in question was non-Hispanic Blacks as compared to non-Hispanic Whites. This significant ethnoracial difference in the rate at which earlier violent arrests produced later investigatory stops appeared in Period 1 as well.

Additionally, and also in line with the first reporting period, benchmarked stops were less likely in predominantly Black non-Hispanic communities, and socioeconomically affluent communities. The effect of ethnicity, however, did not reach statistical significance.

We also compared the size of the ethnoracial effects of the current reporting period to the last six months of the first reporting period (January – June 2016). Models from this period indicated that the ratio of Black stops per violent arrests exceeded that of Whites by 125%; and that the Hispanic stop ratio exceeded that of Whites by 29%. That said, results from the current period may represent an encouraging shift in ethnoracial stop differentials because these differentials are smaller. This is a *descriptive* result, and the statistical significance of the difference in impact between the first and last half of calendar year 2016 was not tested.

Purpose

This report analyzes investigatory stops conducted by the Chicago Police Department from July 2016 to December 2016 (Period 2 under the Agreement).

It focuses on stop counts for three ethnoracial groups: non-Hispanic Whites, non-Hispanic Blacks, and Hispanic Whites (hereafter referred to simply as Hispanics). These aims are addressed using a two-step process. [1]

First, the report provides descriptive statistics of race/ethnicity-specific stop counts and rates for the city of Chicago, and each police district for the 6-month time series. These counts are supplemented with district-level maps displaying the spatial arrangement of stop rates for select months from either the beginning or the end of the period.

Second, the report examines the relationship between ethnoracial-specific arrest counts, in a police district, in the previous month, and ethnoracial-specific stop counts in that same district in the month following. Stated differently, for each of the three ethnoracial groups the ratio of later stops to earlier arrests is considered. In essence this arrangement permits examining "whether stop rates … exceed what we would predict from knowledge of the crime rates of different racial [and ethnic] groups" (Gelman, Fagan and Kiss 2007:815). The arrest variables are essentially benchmarking variables that also allow turning stop counts into stop rates, that is, stops per X many earlier violent arrests. *Both* the numerator and the denominator in this analysis are specific to one of the ethnoracial groups in question. That is, earlier arrests are **also** ethnoracial-specific.

[1] This report focuses on the three mutually exclusive racial/ethnic groups that are most prevalent in Chicago: non-Hispanic Whites, non-Hispanic Blacks, and White Hispanics, Stops associated with other races or ethnicities, including a small number of Black Hispanics, are dropped from the analyses.

The arrests are considered in two different forms: total arrests and only violent (serious Part I) crime arrests. Using different arrest variables as the benchmarking variable alters the meaning of the resulting stop rate. In the former case analyses examine the number of later investigatory stops produced per X number of previous total arrests. In the latter, analyses consider the number of later stops produced per X number of earlier violent arrests.

The implicit rationale for using violent arrests in addition to total arrests as the benchmarking variables is the following. An officer deciding to make a violent crime arrest arguably exercises less discretion than an officer deciding to make any type of nonviolent crime arrest. Since the nonviolent arrests will make up a sizable fraction of total arrests, that total arrest number, as a benchmark, has more officer discretion built into it while, by contrast, the violent arrest number has less officer discretion built into it.

Of central interest is whether those ratios of (later stops/earlier arrests) are different for the three groups. Stated differently and more specifically:

> At the district level, are arrests earlier producing more stops later for non-Hispanic Blacks as compared to non-Hispanic Whites, and for Hispanics as compared to non-Hispanic Whites?

The ethnoracial links between earlier arrests and later investigatory stops are sometimes considered while controlling for changes over time and for differences in community demographic structure across different police districts.

Models will use only use ethnoracial-specific counts when examining the ecological connection between earlier arrests and later stops. The same race and ethnicity combinations appear in **both** the stop count and the arrest count. This in effect creates **ethnoracial-specific *areal* rates** when one of the arrest variables is used as the benchmarking variable.

It bears mentioning that these are *locations* where arrests and stops take place, *and no assumptions are made about the contribution of residents in each district to either the arrest or stop counts*. Arrestees can be arrested in districts where they do not live, and detainees can be stopped in districts where they do not live. It is well known that offenders travel, sometimes substantial distances, to commit crimes like selling drugs or buying drugs. See St. Jean (2007) for a Chicago example of drug sellers commuting to Bronzeville, or Johnson et al. (2013) for an example of people traveling to buy drugs. How much information an investigatory rate carries about *residents* of those locations, is unknown. A similar problem afflicts crime rates and arrest rates (see Taylor 2015: 48-52).

Analyses with one non-ethnoracial-specific benchmark variable appears as well. Some models use the age-weighted population as denominators. Different age segments of the residential population are weighted based on how much that age segment contributes to the volume of detainees across the city. The latter approach assumes, in light of criminological knowledge on the age-crime curve (Gottfredson and Hirschi 1990), that a larger youthful population will result in more stops because this population has higher rates of criminal participation.

Methodology

Stop data were derived from the Investigatory Stop Report forms (ISRs) of the Chicago Police Department for the period July 1, 2016 to December 31, 2016. For the city-wide rate maps describing groups and changes over time, stop counts were aggregated by months, within districts, by ethnoracial combination. Using population of all ages, specifically for the three groups of interest, rates were computed per 1,000 residents. Next, race and ethnicity-specific total arrest and violent arrest counts were matched with each month of stop data, time-lagged by one month.

Demographic data were compiled to account for the major demographic structural ways in which districts may vary. Composite variables were extracted from the 2011-2015 U.S. Census American Community Survey at the census block group level and aggregated to districts. The process of aggregating census block group count data to spatially incongruent units such as police beats and districts is known as areal interpolation. This process entails using a geographic information system (GIS) to extract a value, for every block group, for a variable relative to each block group's contribution to a police beat and district. Area was used as the contribution indicator. GIS was then used to cut portions of block groups that form the area of beats and districts. The proportion of area was measured within each beat and district that truncated block groups compose, and weighted values were computed. Values were then summed across truncated block groups within beats and districts to create new measures (Ratcliffe and McCullagh 1999; Zhang and Qiu 2011). A description of steps taken to interpolate the demographic data is shown in APPENDIX A.

Following the interpolation of demographic data to districts, index measures of socioeconomic status and residential stability were computed. Socioeconomic status represents the standardized average of the following variables: percentage of households with incomes less than $20,000 (reverse factored), percentage of households with incomes greater than $50,000, natural log median home value, and natural log median household income. Residential stability is the average of three standardized values: the percentage of owner occupied households, the percentage of owner occupied housing units occupied by current residents before 1990, and the percentage of renter occupied housing units occupied by current residents before 2000.

Methodological Changes for the 2nd Reporting Period

In response to discussions around limitations of the ecological report for the 1st reporting period, several changes were made for the 2nd reporting period.

Spatial Empirical Bayesian Smoothing

In order to conduct the ecological analysis for period two, we needed to be able to compute ethnoracial-specific stop counts per violent arrests, by district, by month. We learned from the first reporting period that monthly ethnoracial-specific district violent arrest measures—the denominators—are zero-inflated. This posed a fundamental mathematical problem as any number divided by zero is undefined. We addressed this problem using a two-step process. First, we transformed the measure by increasing all monthly ethnoracial-specific violent arrest counts by 1. This effectively removed all zero counts such that stop rates could be calculated for all district-months for each of the three ethnoracial groups.

Second, a spatial Empirical Bayesian smoothing approach reduced the number of extreme outliers in the ethnoracial-specific monthly violent arrest count dataset. This was done by calculating the average ethnoracial-specific violent arrest count for each district's neighboring districts plus the focal district. Adjacent districts with higher margins of error were then adjusted so that their violent arrest counts more closely reflected the area mean. We analyzed a series of box-and-whisker plots before and after the smoothing approach. Results, displayed in

APPENDIX B suggest that the technique substantially reduced the presence of extreme outliers.

Our measure of total arrests also demonstrated zero counts for specific ethnoracial groups over discrete months. We transformed that measure by adding 1, but did not use the spatial Empirical Bayesian smoothing technique.

Spatial Lag

Among the most critical limitations of the ecological analysis for period 1 was that the models did not control for spatial lag effects. This was an important omission, considering that criminological literature has noted that crime and justice outcomes of places are often influenced by their spatial neighbors. There are two general explanations for this phenomenon. First, administrative boundaries (such as police districts and beats) are permeable. In other words, offenders may (knowingly or unknowingly) cross such boundaries while engaging in deviant behavior. Examples of this may include violent disputes over nearby drug territories (Taniguchi, Ratcliffe and Taylor 2011), and/or retaliatory homicides (Kubrin and Weitzer 2003). Second, it is possible that crime and justice outcomes of nearby or adjacent spatial units are influenced by the same underlying sociological or institutional processes. For example, multiple studies have shown that segregated, socioeconomically disenfranchised neighborhoods tend to cluster spatially (Peterson & Krivo, 2010). In turn, shared elevated levels of disadvantage *across* those neighborhoods tend to drive comparably high levels of crime and deviance in those same neighborhoods. Likewise, high stop counts in districts could be associated with stop counts in *nearby* districts if police personnel of all adjacent districts are attempting to respond to elevated levels of violence through the use of investigatory stops.

In the current reporting period, we attempted to control for this possibility by including a spatial lag measure in all conditional models. The spatial lag represents the monthly average stop count for each district's *adjacent* districts. That measure reflects stop counts of all races and ethnicities. In other words, it is not ethnoracial-specific. It just controls for the *overall* amount of police investigatory stop activity nearby.

Age-Weighted Population

In ecological models for the first reporting period, we included a denominator measure of the young population, aged 15-29 years. During communication dated October 4, 2017 the ACLU-IL expressed a preference for a *total* population-based denominator to be used in the current reporting period analyses.

To that end, we included a set of models that uses the age-weighted district total population as a baseline for the creation of district-level stop rates. Weighted variables are common in survey research whereby it is often impossible to study an entire population. As such, survey responses for particular demographic groups are *weighted* such that they approximate representation in the general population.

A large body of criminological research has demonstrated that the likelihood of contact with the criminal justice system peaks in the mid-teens to twenties age-range (Gottfredson and Hirschi 1990; Laub and Sampson 2003; McCall, Land, Dollar et al. 2013). For example, our analysis of Chicago stop data from July to December 2016 revealed that 53% of males and 47% of females stopped were between the ages of 15 and 29. With this in mind, we created a total population-based measure that reflects the age breakdown of those stopped during the second reporting period. More specifically, the reasoning is that of all the population in a locale, some are more at risk of being detained in an investigatory stop than others, based on their age, and we want to incorporate this differential contribution to the population at risk in the population benchmarking variable. An 82 year old in District D is less likely to be detained in an investigatory stop than a 28 year old. Therefore the number of people in a district aged 82 should contribute less to the population at risk of being detained than the number of people in a district aged 28. We also know from the Period 2 post stop outcomes analysis that the detainees are about 85 percent male and 15 percent female. So, ignoring age, out of 100 males in a district 85 of them should contribute to the population at risk of being detained, and out of 100 females, 15 of them should contribute to the population at risk of being detained. Therefore, the *relative* contribution to detainment, based on period 2 information about all detainees, was taken into account for different age segments of the resident population. The same weights for each age segment were applied to the male population and the female population separately. Then, at the end, the exposure-based male and female population counts were weighted relative to each other. We end up with an age and gender weighted population exposed to detainment based on the age profile of those detained citywide during period 2. Calculations for the age-weighted population measure are shown in APPENDIX C.

Analysis

Since the dependent variable represents district-level monthly stop counts, we performed model estimation using count models. [2]

The nesting of stop counts over time within districts, however, calls for multilevel negative binomial modeling. This is because the stop count in a certain district in a certain month is likely to be strongly linked to the stop count in that district in both the months preceding the certain month and following the certain month. The multilevel model variation adjusts estimates and error terms for within- and between-group scores, considering the likelihood that observations within districts are more likely to be similar than between-district observations (Snijders and

[2] Count models such as Poisson regression are appropriate for data with a Poisson distribution (Osgood 2000). Poisson models assume that the outcome variable has a mean and variance that are roughly equal. The condition of overdispersion occurs in instances where the variance exceeds the mean. Yet, overdispersion can be accommodated by adding an additional term to the model function. Due to the presence of overdispersion in the data (mean = 127.88, variance = 33,168.78), negative binomial regression is appropriate to model stop counts.

Bosker 1999). Failing to do so would undermine the assumption of independent error terms. All models were fitted using Stata's MENBREG (Mixed Effects Negative Binomial Regression).

As a type of count model, MENBREG requires the use of an exposure variable to normalize observed events relative to their opportunities for occurrence, i.e., the population at risk of exposure to the outcome. For example, one could collect data on the number of individuals diagnosed with Alzheimer's disease across Chicago neighborhoods. But, to examine relative differences across neighborhoods a researcher also needs to select an appropriate denominator to compute prevalence rates. As such, an appropriate denominator might be the number of elderly residents, considering the association of age with the disease. In modeling stop counts we have taken note of ongoing scholarly discussion regarding the use of denominator measures (Fagan 2002; Ridgeway and MacDonald 2010; Walker 2001).

As mentioned above, three different exposure variables were used for three different model series. Those exposure variables were monthly violent arrest counts for each of the three major ethnoracial groups of interest, monthly total arrest counts for each of the three major ethnoracial groups of interest, and age-weighted, then gender-weighted population, **regardless of race or ethnicity**. The first two exposure variables can vary from month to month. The last one, age-weighted population, is constant within each district for the entire period.

The units of analysis were district-months or more specifically, monthly stop counts nested within police districts. In other words, each of Chicago's 22 police districts has 6 monthly observations (July 2016 – December 2016) for non-Hispanic Black, non-Hispanic White, and Hispanic stops. This computes to a total of 396 district-month-ethnoracial-specific observations. The models only consider stops of the three ethnoracial groups identified in the consent agreement. [3]

Results

Monthly Stop Counts and Rates

Table 1 displays total monthly stop counts and rates for all ethnoracial groups (summed), as well as specific counts and rates for three ethnoracial groups of interest: non-Hispanic Blacks, non-Hispanic Whites, and Hispanics. Stop rates were calculated by dividing monthly counts by the residential population of any given group, multiplied by 1,000. Thus, stop rates can be interpreted as the expected number of stops of a given ethnoracial group (or total population) per 1,000 residents of said ethnoracial group (or total population).

A total of 51,248 investigatory stops occurred from July through December 2016.[4] During that six-month period, 71% of those stops were of non-Hispanic Blacks, 8.4% were of non-Hispanic Whites, and 19.4% were of Hispanics. The overall (All) average monthly stop rate was 3.15 per

[3] As reported in the post stop outcomes report for period 2 (Taylor and Johnson, 2017: Table 1), detainees in these three ethnoracial groups comprise 98.4 percent of all detainees in investigative stops.

[4] This number excludes 18 stops missing district information, as well as 86 stops with a district code of 31 or 41.

1,000 residents. Among ethnoracial groups, the average monthly stop rate for Blacks was the highest at 7.23, followed by Hispanics (3.70), then Whites (0.82).

To draw descriptive comparisons that control for seasonality effects, the average monthly stop rates of the current reporting period were compared with the average monthly stop rates from the last six months of 2015. Overall, the average monthly rate of stops per total population decreased by 81.32% from July to December 2015 to the second reporting period. Similar percentage decreases were evident in the stop rates of ethnoracial groups: Blacks (-81%), Whites (-82%), and Hispanics (-79%).

Table 1: City-Level Ethnoracial-Specific Stop Counts and Rates, by Population

Month	Counts				Rates			
	All	Black	White	Hispanic	All	Black	White	Hispanic
July	9,600	6,699	908	1,873	3.54	7.99	1.04	4.18
August	8,853	6,360	770	1,618	3.26	7.58	0.88	3.61
September	9,261	6,584	762	1,813	3.41	7.85	0.87	4.04
October	9,522	6,555	802	2,046	3.51	7.82	0.92	4.56
November	7,647	5,496	576	1,492	2.82	6.55	0.66	3.33
December	6,365	4,705	471	1,110	2.35	5.61	0.54	2.48

Line graphs of monthly stop counts and rates per 1,000 population appear in Figure 1 and Figure 2, respectively. Each line represents stops of a one of three ethnoracial groups of interest, or stops of individuals of any given race or ethnicity. Figure 2 shows that the overall (any race or ethnicity) stop rate for the city of Chicago in July of 2016 was 3.54 per 1,000 residents. That rate decreased just slightly through August (3.12) before increasing again in September (3.41) and October (3.51). A more substantial decrease, however, was demonstrated throughout the rest of the year. By December 2016 stop rates bottomed out at 2.35 per 1,000 residents.

Stops of specific ethnoracial groups generally followed the overall (Any) trend. For example, stop rates of Blacks, Whites, and Hispanics also demonstrated slight decreases between July and August. Additionally, all three ethnoracial groups experienced their most notable rate decreases between October and December. For example, the stop rate decreased by 46% for Hispanics (4.56 to 2.48) and 28% for non-Hispanic Blacks (7.82 to 5.61). Over the same period, the rate of stops of non-Hispanic Whites decreased by 41% (0.92 to 0.54).

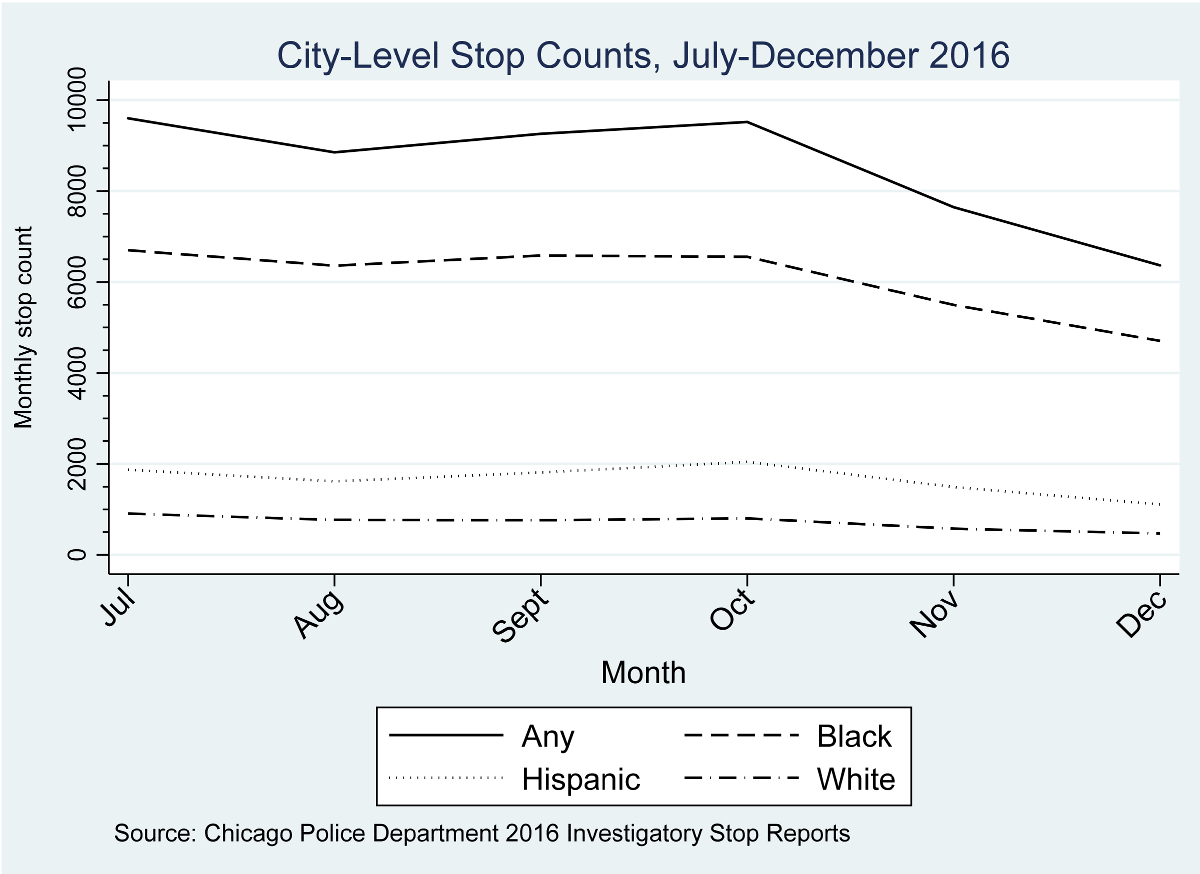


Figure 1: City-Level Stop Counts, July-December 2016

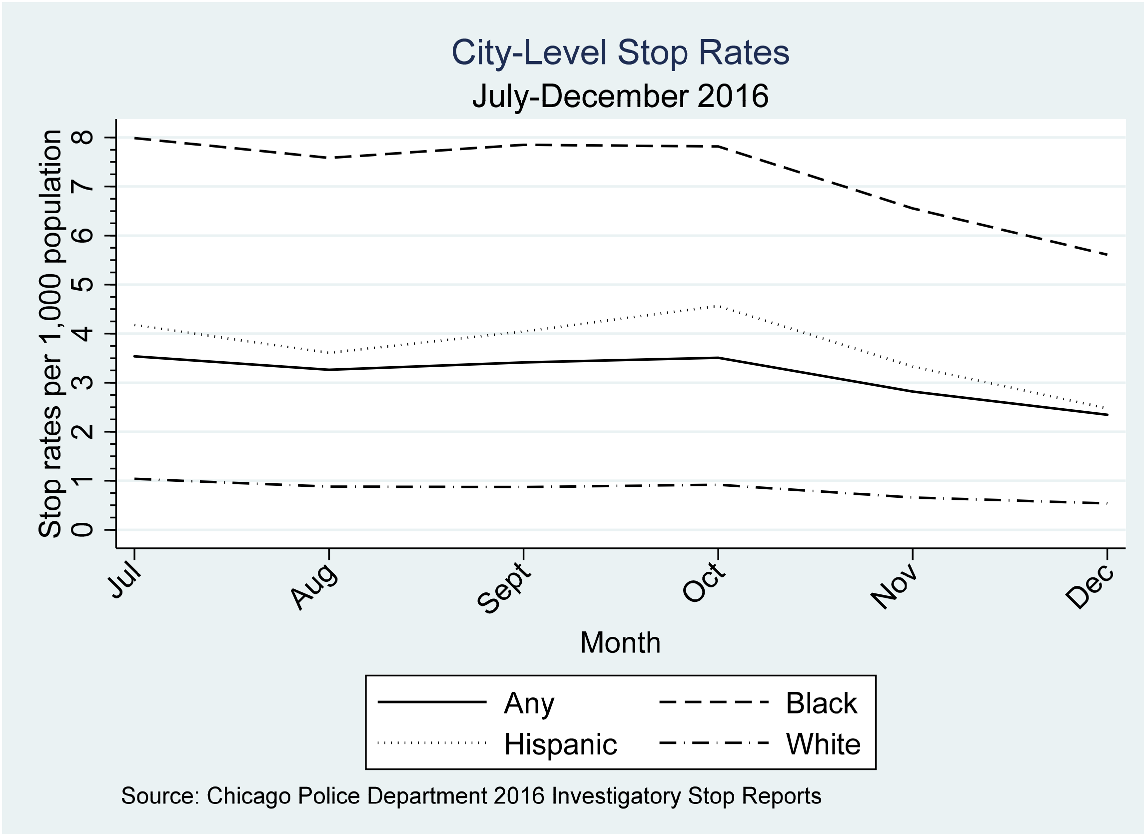


Figure 2: City-Level Stop Rates by 1,000 Population

Figure 3 displays city-level ethnoracial-specific and total stop rates per 100 previous month's violent arrests. Thus, whereas Figure 2 used the ethnoracial-specific residential population for the creation of stop rates, the denominator of Figure 3 reflects the total number of ethnoracial-specific violent arrests in the month prior. Violent arrests were measured as the sum of homicides, robberies, and aggravated assaults. Overall, about 3,400 stops per 100 violent arrests occurred in July of 2016. That stop rate peaked at 3,827 in September, before decreasing steadily through December and bottoming out at 2,893 per 100 violent arrests the month prior.

When the denominator was violent arrests from the previous month, each ethnoracial group demonstrated a unique pattern of stop rates over time. Stop rates of non-Hispanic Blacks and Hispanics were comparable from August through October of 2016, with divergence thereafter. As an example, the stop rates per violent arrest for Blacks and Hispanics was over 3,100 in September. Yet, the Hispanic stop rate decreased to about 2,000 by December, while the Black rate dropped by less, to 2,479 for the same month. Stop rates of Whites contrasted the patterns of the other two ethnoracial groups. While rates of Black and Hispanic stops peaked in the summer months, stops of Whites actually decreased (2,420 in July to 1,934 in September). In general, stops of the Black and Hispanic ethnoracial groups decreased and leveled off from October through December. Out of all three ethnoracial groups, stops of non-Hispanic Blacks most closely mirrored the overall (Any) trend. This is understandable in part because this group made up about 70 percent of all stops. One last point. It is interesting that the non-Hispanic Black and Hispanic stop rates prove closely comparable from August through October but diverge in the month before and month after that.

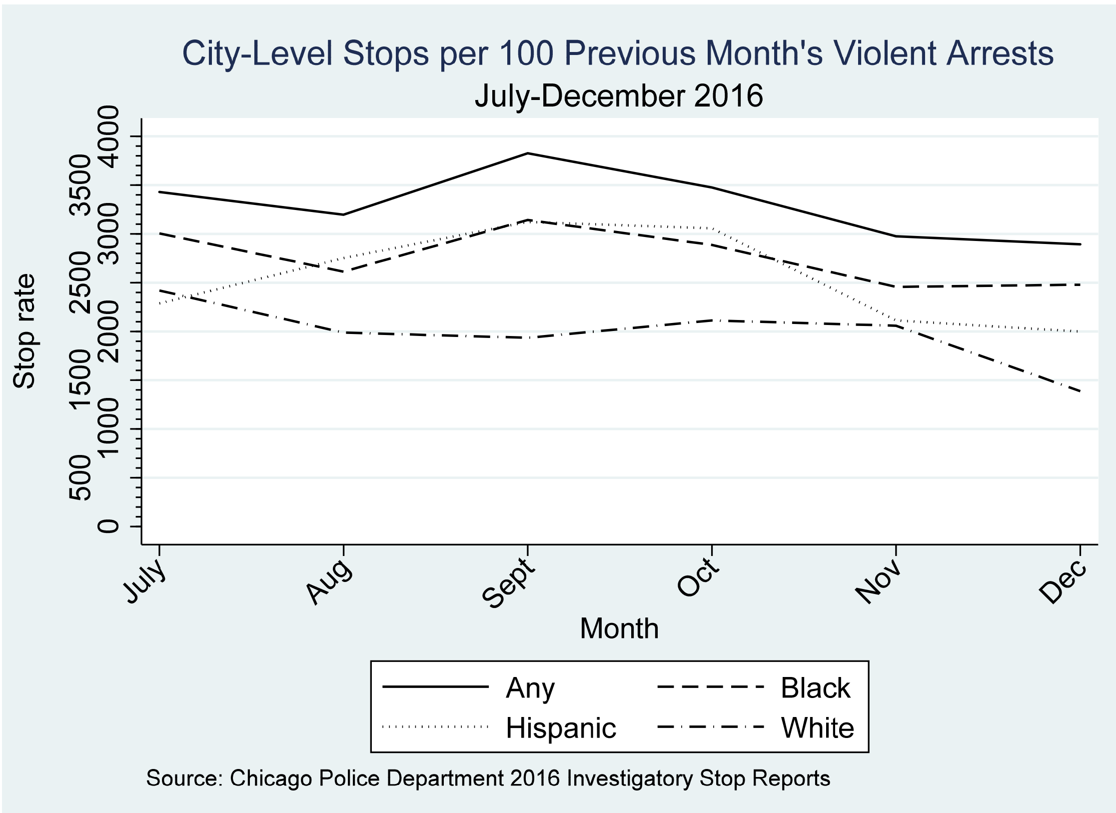


Figure 3: City-Level Stops per 100 Previous Month's Violent Arrests

A line graph of stops per previous month's 100 total arrests is shown in Figure 4. Total arrests refer to arrests for any kind of offense. Worth noting is that for all four groups the ratios of stops per total arrests (Figure 4) are substantially smaller than the ratios of stops per 100 violent arrests (Figure 3).

Across all ethnoracial groups (the Any trendline), the ratio of stops per previous month's total arrests began at 121 stops per arrests in July and peaked at 136 stops per arrests in September. The ratio then decreased steadily through December with an ultimate low of 101 stops per 100 previous arrests. The ratio of stops per 100 previous total arrests of non-Hispanic Blacks followed the Any trendline so closely that both trendlines are almost indistinguishable at select points of the time series. The ratio of stops per 100 total arrests for Hispanics most greatly exceeded the general trend. By the month of October, the Hispanic stop ratio was 163 stops per arrests, compared to the Any ratio of 135 stops per arrests. That disparity decreased throughout the remaining months. By December the Hispanic ratio of stops per arrest was 110 stops per arrest, compared to 101 for the ratio of stops per total arrests for individuals of any racial or ethnic group. Stop ratios for Whites were comparatively lower than the general trend and the two other ethnoracial groups. Similar to Figure 3, Figure 4 shows that the White stop ratio decreased in the summer months, demonstrated an uptick in October (131), and then decreased again to 89 in December.

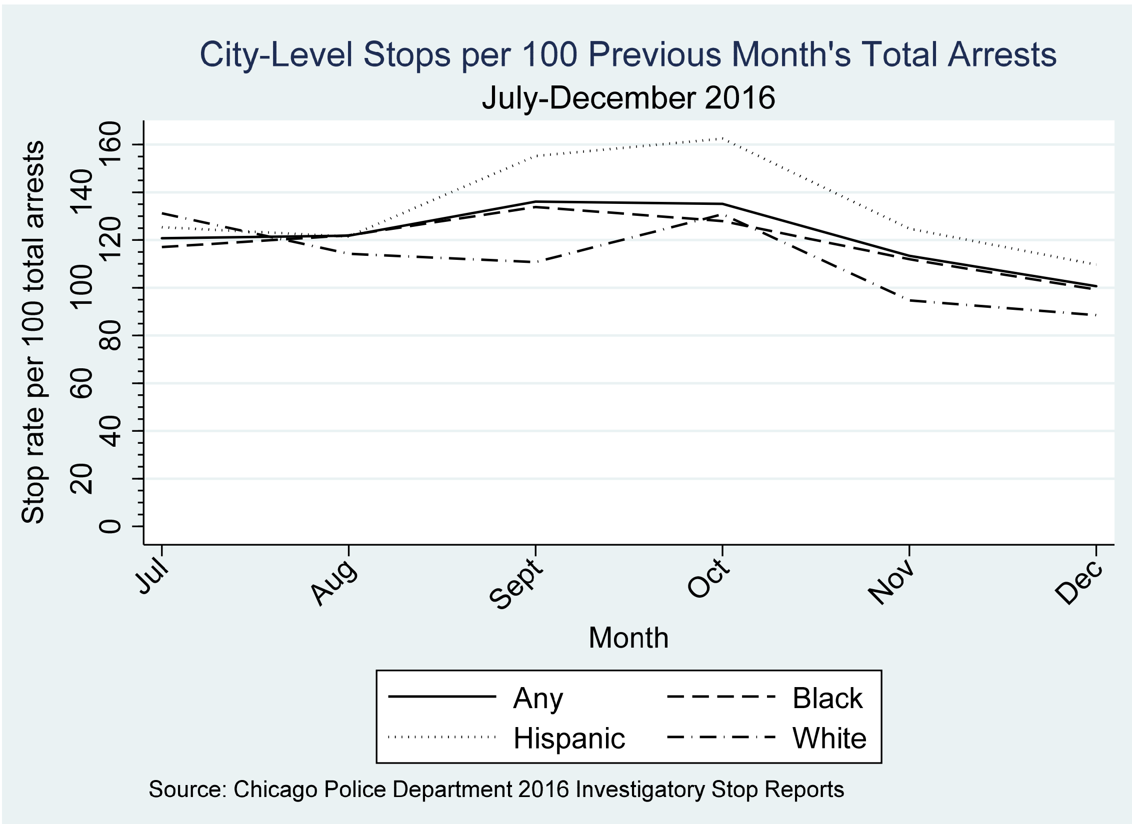


Figure 4: City-Level Stops per Previous Month's Total Arrests

City-level stops per 100 violent and total arrests are shown in Table 2.

Table 2: City-Level Stops per 100 Previous Month's Arrests

Month	Violent Arrests				Total Arrests			
	All	Black	White	Hispanic	All	Black	White	Hispanic
July	3,428.6	3,005.3	2,419.8	2,287.3	120.7	117.1	131.2	125.4
August	3,196.0	2,613.4	1,988.0	2,752.6	121.8	122.0	114.2	121.5
September	3,826.9	3,143.1	1,934.4	3,122.4	136.1	133.8	110.8	155.2
October	3,475.2	2,886.3	2,112.5	3,057.2	135.2	128.0	130.8	162.5
November	2,975.5	2,456.4	2,058.7	2,112.7	113.4	112.0	94.7	124.7
December	2,893.2	2,479.0	1,387.7	1,999.5	100.7	99.3	88.5	109.7

District-level monthly stop counts and rates per 1,000 population are shown in Appendix B. District-level monthly stop rates per 100 previous month's violent and total arrests are shown in Appendix C.

Maps of District-Level Monthly Stop Rates

Thematic maps display data associated with places. In this case, each thematic map details district-level stop rates for one month, for one of the three ethnoracial groups of interest. Rates were computed per 1,000 residents of the given ethnoracial group for each map. Moreover, each map is organized by quantiles, with each quantile representing 20 percent of Chicago's 22 districts under study. Quantile categories are organized on a monochromatic scale with higher stop rate districts represented by darker, as opposed to lighter shading. More specifically, districts with ethnoracial stop rates falling within the lowest quantile are displayed in white, and districts with ethnoracial stop rates falling within the highest quantile are displayed in black. The 31st district (denoted by the cross-hatched features of the map) is excluded since stops in those areas occurred outside of the jurisdiction of the Chicago Police Department. All maps are displayed in Appendices D - U.

Non-Hispanic Black Stop Rates

Throughout much of the six-month time series, stop rates of non-Hispanic Blacks appeared to be the highest in the West and Near South of the city. For example, the 7th, 9th, 10th, 11th, and 15th districts scored in the middle to highest quantiles from July through December. Almost all of the aforementioned districts scored at or above the 60th percentile on stop rates for a given month (top two quantiles) except for the 10th district in July, and the 7th district in December. Another cluster of districts with higher stop rates appear in the Northside of the city. This cluster consistently included the 16th, 18th and 19th districts. The 17th district only emerged in the second to highest quantile in July.

By contrast, the lowest stop rate districts were clustered in the South side of the city. These included the 3rd, 6th, 8th, and 22nd districts. While the 4th (in October and December) and 5th districts (in September) reached the middle quantile in select months, rates there are also comparatively low. The lowest Black stop rates also emerged in the 12th district in August, the 20th district in October, and the 1st district in November.

Non-Hispanic White Stop Rates

The spatial arrangement of non-Hispanic White stop rates demonstrated substantial consistency over time. For example, from July through December 2016 the 1st, 2nd, 12th, 14th, 17th, and 19th districts fell in the lowest two quantiles of the stop rate distributions. During August and from October through December, the 3rd district also fell within the lowest quantile. Similarly, White stop rates in the 22nd district fell within the lowest quantile for all months of the time series. There was a ribbon of districts that fell within the city's middle to highest quantile of stop rates sandwiched between the 22nd district, and the aforementioned low White stop rate districts. Although there was a degree of variation from month to month, some of these districts include the following: 4th, 5th, 6th, 7th, 9th, 10th, 11th, 15th, and 25th.

Hispanic White Stop Rates

From July through December the 7th, 9th, 11th, and 15th districts were within the top two quantiles for stop rates of Hispanics. For the last three months of the year, the 6th district also appeared to have stop rates in the highest quantile. And for five out of six months (July through November), rates of the 12th district also appeared in the highest two quantiles. Elevated rates also emerged in the 1st district in September and October; and in the 24th, 20th, and 19th districts to the north, particularly in July, August, and December.

Two general areas of the city concentrated low stop rates for Hispanics. The first is the Northside. The 16th, 17th, and 14th districts consistently scored within the lowest two quantiles from July through December. At times, the 18th (July) and 1st districts (August and November) emerged within the lowest quantile as well. The Southside also displayed monthly rates that fell within the lowest quantile categories. For example, the 22nd district demonstrated among the lowest stop rates for Hispanics throughout the entire period. Stop rates within the 8th district were also low for most months, save November. Otherwise, additional low-stop rate districts were temporally sporadic. These included the 6th, 5th,4th, 3rd, and 2nd districts.

Inferential Models

ANOVAs

The analysis of variance (ANOVA) or unconditional models without predictors indicated statistically significant between-district variation in monthly stops counts. This finding suggested the need for multilevel modeling. Moreover, this finding held regardless of the exposure measure included in the model. [5] In each ANOVA model, the IRR represents the incidence rate ratio, or expected average count per exposure unit, across all three ethnoracial groups, over the entire period, in the average district. More specifically, we could say the following after data adjustments made by a given statistical model:

- In a typical district, in a typical month during the period, across all three focal ethnoracial groups, there were on average 20 stops per violent arrest in that district the previous month (IRR=20.31);
- In a typical district, in a typical month during the period, across all three focal ethnoracial groups, on average, there was about 1 stop per arrest (of any kind) in that district the previous month (IRR=1.17); and
- In a typical district, in a typical month during the period, across all three focal ethnoracial groups, there was an average of approximately .03 stops for every 1 person

[5] In these models the exposure variable is per individual unit, for example per individual violent arrest, rather than a metric for the earlier figures that used per 100 total arrests or violent arrests. Note also that in the case of violent arrests, the number is based on an adjusted average for the focal district and the immediately surrounding districts.

in the age-then-gender-weighted general population (IRR=0.03). More literally, about 1 stop occurred for every 33 Chicago age-then-gender-weighted residents where the weighting aligns with detainment risk.

Model Series with Violent Arrests as Exposure Measure

The first model series employs spatial Empirical Bayes smoothed violent arrest counts as the baseline. This transformed stop counts into rates per the previous month's spatially smoothed violent arrest counts.

Table 3 displays the results of multilevel count regression models used to regress stop counts on ethnoracial indicators (non-Hispanic Black vs. non-Hispanic White, Hispanic vs. non-Hispanic White), while controlling for temporal changes and demographic structure. Model A uses two dummy predictors to consider the extent to which stop counts of non-Hispanic Blacks and Hispanics differ from non-Hispanic Whites. Non-Hispanic Whites represented the reference group against which the other two ethnoracial groups were compared. That said, IRRs (incidence rate ratios) represent the factor by which stop counts are expected to change based on a one-unit increase in any given predictor. Because the non-Hispanic Black and Hispanic variables are coded 0/1, the IRRs for each indicate the extent to which the expected stop counts for these groups differ from non-Hispanic Whites (the reference group), controlling for additional correlates in the model. Model A also included a spatial lag measure. This can be interpreted as the predicted change in stop counts based on a one unit increase in the average stop counts of each district's *adjacent* districts in any given month.

These tables are organized for each variable as follows.

The impact coefficient appears first (e.g., .0004). This is in a log metric.

The standard error for that coefficient follows immediately below.

Below that, in italics, is the t-test statistic. This represents the ratio of the impact/ (standard error of impact). For example, .0004/.0005 = .8. If a t-test generates a value greater than |1.96|, it is statistically significant at the .05 level (less than five in a hundred chance that differences are just due to chance); greater than |2.576| is statistically significant at the .01 level (less than one in a hundred chance that differences are just due to chance), and |3.291| is significant at the .001 level (less than one in a thousand chance that differences are just due to chance). All significance tests are two tailed.

Below that the impact appears in incident rate ratio (IRR) form. For predictors, when the IRR is above 1, this indicates the expected percent impact on the outcome count with a one-unit increase in the predictor. If the IRR is below 1, the number must be subtracted from 1 to arrive at the expected percent decrease associated with a one-unit increase in the predictor.

Model A indicates that non-Hispanic Black stop counts were predicted to exceed non-Hispanic White stop counts across the 6-month reporting period by roughly 79% (IRR=1.79). This finding was statistically significant—surpassing the odds of chance ($p<.001$). In other words, violent crime arrests occurring within a district and involving non-Hispanic Blacks generated substantially more investigatory stops of persons in the same ethnoracial group, the following month, in that district compared to the stop/arrests for violent crimes ratio associated with non-Hispanic Whites.

Similarly, stop counts of Hispanics were also predicted to exceed those of non-Hispanic Whites. Specifically, expected Hispanic stop counts exceeded those of non-Hispanic Whites by about eight percent (IRR=1.08). That finding, however, was not statistically significant. Statistically speaking, the results of the two-tailed t-test did not allow us to reject the null hypothesis that stop counts of non-Hispanic Whites and Hispanics were relatively equal. The rate at which Hispanic violent crime arrests generated later Hispanic stops was roughly close to the rate for non-Hispanic Whites.

Model A also considered whether district stop counts were influenced by investigatory stop activity in nearby districts. The spatial lag measure indicated that a one-unit change in the average stop counts in adjacent districts was associated with an expected increase in stop counts of much less than one percent. Yet, that finding did not amount to statistical significance. Stop rates in a focal district were *not* significantly influenced by stop rates in nearby districts.

Model B retained all predictors included in Model A, but added a temporal measurement. Time was a linear sequence variable that represents each of the six months of the second reporting period (July through December 2016). The purpose of adding the effect of time was 1.) to see if monthly stop counts demonstrated change throughout the time series, and 2.) to determine if ethnoracial effects persisted when controlling for time. Model B indicated that for every one-unit increase in time, that is, each additional month, expected stop counts decreased by about 6 percent (IRR=.94). This finding was statistically significant ($p<.05$). Controlling for time did not substantially alter the effects of ethnoracial dummy predictors on stop counts. As seen before in Model A, here too non-Hispanic Black stop rates per earlier violent arrest were expected to exceed the rates for non-Hispanic Whites by almost 80% (IRR=1.80, $p<.001$). The Hispanic White and spatial lag measures remained statistically non-significant influences.

Model C controlled for community demographic structure by including measures of the proportion of non-Hispanic Black residents, socioeconomic status, and residential stability for each police district. The addition of these measures did little to alter demonstrated ethnoracial differences in district-level stop counts. In Model C, expected stop rates per violent arrest of non-Hispanic Blacks exceeded those of non-Hispanic Whites by a factor of 1.82 or 82 percent (IRR=1.82, $p<.001$). Differences between Hispanic and non-Hispanic White stop rates remained statistically negligible. Every one-unit increase in time was associated with a 6 percent decrease in expected stop rates (IRR=.94, $p<.05$).

Noteworthy structural effects appear in Model C. First, there was a significant negative effect of district racial composition on stop counts. A one-unit increase in the proportion of non-Hispanic

Black residents was associated with an 83 percent decrease in expected stop rates per violent arrest —a finding that was statistically significant at the highest level (IRR=.17, $p<.001$). Generally, in more predominantly Black non-Hispanic districts, earlier violent arrests produced fewer subsequent stops. Furthermore, investigatory stops were less likely to occur in socioeconomically affluent districts. For every one-unit increase in district socioeconomic status, stop counts decreased by almost 40 percent (IRR=0.61, $p<.05$). While the effect of residential stability was positive (IRR=1.14), it did not amount to statistical significance.

That said, structural effects at the district level must be interpreted with extreme caution, given the difficulties associated with such a small number of districts in these mixed effects models (Bryan & Jenkins, 2016).

How exactly do the current findings compare with those of the first reporting period? To answer this question, we compared the IRR values of Model C with a parallel model capturing the last 6 months of the first reporting period. The IRR race effect from the first reporting period yielded a value of 2.25. In other words, when considering the last six months of that period, stops of Blacks per violent arrest exceeded stops of Whites per violent arrest by a factor of 2.25 or 125%. For the current reporting period, stops of Blacks per violent arrest exceed those of Whites by 82% (Model C). (We have not calculated the confidence interval associated with each IRR to gauge if this is a statistically significant drop in the size of the race effect.) Additionally, while stops of Hispanics were predicted to significantly exceed Whites in the first period by 29% (IRR=1.29, $p<.01$), that effect does not reach significance in the current reporting period. The effects of district racial composition and socioeconomic status remain important across both periods in the prediction of ethnoracial stops, and are consistent in effect size and direction. Across both periods, earlier violent arrests generate fewer stops later in more predominantly Black non-Hispanic districts, and in higher SES districts. The model results of the last six months of the first reporting period can be found in APPENDIX Z.

Models D and E considered the robustness of temporal findings through the use of alternative measurements. Toward this goal, in Table 3, Models D and E substituted the previously-described linear measure of time for 5 monthly dummy indicators. July of 2016 was the reference time period.

Ethnoracial impacts described thus far persisted in Model D. Stop rates of non-Hispanic Blacks exceeded those of non-Hispanic Whites by 80 percent. Stop rate differences between Hispanics and non-Hispanic Whites remained non-significant. The introduction of monthly temporal dummy measures revealed that significant decreases in stop counts occurred in November and December of 2016. Compared to stop rates of non-Hispanic Whites in July, stops rates were 24 percent less in November (IRR=.76, $p<.05$) and 34 percent less in December (IRR=.66, $p<.01$). Further, these effects remained similar in statistical significance, direction and relative size, when controlling for the community demographics shown in Model E.

Table 3: Predicting Stop Counts using Violent Arrests as Exposure Measure

	Model A	Model B	Model C	Model D	Model E
Spatial Lag	0.0004	-0.0003	-0.0002	-0.0010	-0.0009
	(0.0005)	(0.0005)	(0.0005)	(0.0006)	(0.0006)
	0.8838	*-0.5519*	*-0.4036*	*-1.5971*	*-1.4134*
	1.0004	***0.9997***	***0.9998***	***0.9990***	***0.9991***
Black	0.5800	0.5861	0.6004	0.5871	0.5987
	(0.0928)	(0.0923)	(0.0926)	(0.0916)	(0.0919)
	6.2518	*6.3519*	*6.4874*	*6.4092*	*6.5131*
	1.7860	***1.7970***	***1.8228***	***1.7988***	***1.8198***
Hispanic	0.0794	0.0872	0.0809	0.0819	0.0761
	(0.0910)	(0.0906)	(0.0906)	(0.0901)	(0.0902)
	0.8720	*0.9629*	*0.8932*	*0.9081*	*0.8434*
	1.0826	***1.0911***	***1.0843***	***1.0853***	***1.0791***
Time		-0.0588	-0.0568		
		(0.0254)	(0.0255)		
		-2.3113	*-2.2312*		
		0.9429	***0.9448***		
Percent Black			-1.7734		-1.8112
			(0.5582)		(0.5692)
			-3.1769		*-3.1821*
			0.1698		***0.1635***
Status			-0.5002		-0.5734
			(0.2362)		(0.2434)
			-2.1180		*-2.3560*
			0.6064		***0.5636***
Stability			0.1329		0.1461
			(0.1500)		(0.1530)
			0.8857		*0.9545*
			1.1421		***1.1573***
August				-0.0782	-0.0765
				(0.1223)	(0.1223)
				-0.6395	*-0.6256*
				0.9248	***0.9264***
September				0.0174	0.0179
				(0.1207)	(0.1208)
				0.1443	*0.1481*
				1.0176	***1.0181***
October				-0.0241	-0.0262
				(0.1202)	(0.1203)
				-0.2004	*-0.2175*
				0.9762	***0.9741***
November				-0.2803	-0.2707
				(0.1354)	(0.1355)
				-2.0706	*-1.9979*
				0.7556	***0.7628***
December				-0.4193	-0.4031
				(0.1569)	(0.1571)
				-2.6728	*-2.5659*

				0.6575	***0.6682***
Intercept	2.5963	3.0058	3.7000	3.2630	3.9625
	13.4140	***20.2024***	***40.4473***	***26.1278***	***52.5886***
Observations	396	396	396	396	396
Number of groups	22	22	22	22	22
Log Likelihood	-2029	-2027	-2022	-2024	-2020
df	3	4	7	8	11
Chi Square	44.67	50.51	59.36	56.65	64.80
BIC	4,094.76	4,095.39	4,104.38	4,113.96	4,123.39

Note. For each model, each predictor, table shows b weight, (standard error of b-weight in parentheses), *t value*, and ***Incidence Rate Ratio (IRR).*** Exposure variable not shown.

Model Series with Total Arrests as the Exposure Measure

Stop count models were run using ethnoracial-specific total arrest count as the exposure variable. Differences emerged when comparing the results of models using total arrests to those using spatially smoothed violent arrests. In Table 4**,** Model A indicated significant ethnoracial impacts. Non-Hispanic Black expected stop rates per total arrest exceeded non-Hispanic White stop rates by about 14 percent (IRR=1.14, p<.01). This difference is notably smaller than the 80% difference revealed in Model A of the violent arrest models. Moreover, Hispanic expected stop rates also exceeded non-Hispanic White stop rates by a factor of 1.13, or 13 percent. And, this effect reached statistical significance (p<.05). Also, a significant but small spatial lag effect emerged (IRR=1.0005, p<.05).

Ethnoracial effects on stop rates remained even when controlling for time, racial composition, socioeconomic status, and residential stability (Model C); however, the effect of stop counts in adjacent districts became statistically irrelevant (IRR=1.0, p>.05). Over time, stop counts per total arrest demonstrated a significant negative trend. For every one-unit increase in time, expected stop rates decreased by about 5 percent (IRR=.95, p<.001). And, in stark contrast to the violent arrest models, none of the community structural correlates demonstrated an effect on expected stop rates.

Additional Models D and E substitute monthly dummy effects for the linear sequence measure, but doing so did not appreciably alter ethnoracial effects described thus far. Both models, however, revealed a small yet significant, *but now negative,* spatial lag effect (IRR=.9988, p<.001). The finding that overall expected rates decreased in the months of November and December is consistent with the violent arrest models, relative to July. The use of monthly time dummy indicators did yield a significant socioeconomic status effect. For every one-unit increase in district socioeconomic status, expected stop rates were predicted to decrease by about 34 percent (Model E IRR=.66, p<.05).

Table 4: Predicting Stop Counts using Total Arrests as Exposure Measure

	Model A	Model B	Model C	Model D	Model E
Spatial Lag	0.0006	-0.0000	-0.0001	-0.0012	-0.0013
	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)
	2.2444	-0.1274	-0.3017	-3.4125	-3.7837
	1.0006	***1.0000***	***0.9999***	***0.9988***	***0.9987***
Black	0.1317	0.1379	0.1393	0.1417	0.1423
	(0.0473)	(0.0467)	(0.0467)	(0.0438)	(0.0438)
	2.7870	2.9533	2.9812	3.2368	3.2497
	1.1408	***1.1479***	***1.1495***	***1.1522***	***1.1529***
Hispanic	0.1246	0.1307	0.1305	0.1303	0.1305
	(0.0499)	(0.0493)	(0.0493)	(0.0463)	(0.0463)
	2.4969	2.6499	2.6457	2.8115	2.8158
	1.1327	***1.1396***	***1.1394***	***1.1392***	***1.1394***
Time		-0.0488	-0.0504		
		(0.0140)	(0.0141)		
		-3.4962	-3.5648		
		0.9524	***0.9508***		
Percent Black			-0.7060		-0.7839
			(0.4201)		(0.4232)
			-1.6805		-1.8521
			0.4936		***0.4566***
Status			-0.2698		-0.4112
			(0.1760)		(0.1784)
			-1.5326		-2.3044
			0.7635		***0.6629***
Stability			0.0562		0.0771
			(0.1135)		(0.1143)
			0.4954		0.6740
			1.0578		***1.0802***
August				-0.0888	-0.0941
				(0.0619)	(0.0620)
				-1.4345	-1.5182
				0.9150	***0.9102***
September				0.0260	0.0226
				(0.0609)	(0.0610)
				0.4267	0.3706
				1.0263	***1.0229***
October				0.0838	0.0834
				(0.0605)	(0.0605)
				1.3837	1.3774
				1.0874	***1.0870***
November				-0.3147	-0.3290
				(0.0709)	(0.0711)
				-4.4399	-4.6275
				0.7300	***0.7196***
December				-0.4492	-0.4721
				(0.0837)	(0.0840)
				-5.3670	-5.6175

				0.6381	***0.6237***
Intercept	-0.1619	0.1931	0.5092	0.6404	1.0270
	(0.1383)	(0.1670)	(0.2457)	(0.1864)	(0.2585)
	-1.1704	1.1564	2.0726	3.4358	3.9727
	0.8505	***1.2130***	***1.6640***	***1.8972***	***2.7927***
Observations	396	396	396	396	396
Number of groups	22	22	22	22	22
Log Likelihood	-1803	-1797	-1796	-1778	-1775
df	3	4	7	8	11
Chi Square	14.11	26.88	29.72	72.69	78.14
BIC	3,642.84	3,636.70	3,651.93	3,621.18	3,634.08

Note. For each model, each predictor, table shows b weight, (standard error of b-weight in parentheses), *t value*, and ***Incidence Rate Ratio (IRR).*** Exposure variable not shown.

Model Series with Age-Weighted Population as the Exposure Measure

Parallel models were also run using the age-weighted then gender-weighted district population as the exposure measure. To be clear, this exposure measure is not ethnoracial-specific. Table 5, Model A regresses stop counts against the ethnoracial predictors, and the spatial lag of the outcome. When using age-weighted then gender-weighted population as the exposure measure, expected stop rates of non-Hispanic Blacks exceeded the reference group by 1,086 percent (IRR=11.86, p<.001). Expected Hispanic stop rates also significantly exceeded expected rates of non-Hispanic Whites, by 89 percent (IRR=1.89, p<.001). The predicted ethnoracial rate differentials remained even when controlling for additional correlates included in Model C.

Structurally, racial composition and socioeconomic status were critical predictors of stop counts per weighted population. Specifically, every one-unit increase in the proportion of non-Hispanic Black residents was associated with a 75 percent decrease in the rate of expected investigatory stops (IRR=.25, p<.001). Socioeconomic status was also associated with decreases in expected stop rates. For every one-unit increase in district status, stop counts decreased by 53 percent. The results of Models D and E, which substituted monthly dummy measures, were relatively consistent with results described thus far.

Table 5: Predicting Stop Counts using Age-Weighted Population as Exposure Measure

	Model A	Model B	Model C	Model D	Model E
Spatial Lag	0.0016	0.0010	0.0003	0.0005	-0.0001
	(0.0005)	(0.0006)	(0.0005)	(0.0007)	(0.0006)
	3.2468	1.7122	0.6667	0.8009	-0.2197
	1.0016	***1.0010***	***1.0003***	***1.0005***	***0.9999***
Black	2.4732	2.4965	2.4770	2.5040	2.4782
	(0.1214)	(0.1210)	(0.1241)	(0.1208)	(0.1234)
	20.3699	20.6245	19.9604	20.7302	20.0834
	11.8603	***12.1399***	***11.9055***	***12.2313***	***11.9198***
Hispanic	0.6370	0.6501	0.6331	0.6493	0.6340
	(0.1138)	(0.1133)	(0.1127)	(0.1130)	(0.1122)
	5.5967	5.7374	5.6170	5.7451	5.6488
	1.8908	***1.9157***	***1.8834***	***1.9142***	***1.8851***
Time		-0.0694	-0.0866		
		(0.0300)	(0.0295)		
		-2.3153	-2.9371		
		0.9330	***0.9170***		
Percent Black			-1.3783		-1.3975
			(0.4283)		(0.4317)
			-3.2179		-3.2376
			0.2520		***0.2472***
Status			-0.7596		-0.8105
			(0.1793)		(0.1828)
			-4.2355		-4.4343
			0.4679		***0.4446***
Stability			0.1381		0.1461
			(0.1113)		(0.1123)
			1.2409		1.3010
			1.1481		***1.1573***
August				-0.1086	-0.1314
				(0.1507)	(0.1500)
				-0.7203	-0.8761
				0.8971	***0.8769***
September				-0.0756	-0.0915
				(0.1498)	(0.1493)
				-0.5047	-0.6127
				0.9272	***0.9126***
October				-0.0775	-0.0788
				(0.1494)	(0.1489)
				-0.5192	-0.5291
				0.9254	***0.9242***
November				-0.2933	-0.3567
				(0.1624)	(0.1594)
				-1.8063	-2.2376
				0.7458	***0.7000***
December				-0.4614	-0.5682
				(0.1821)	(0.1749)
				-2.5336	-3.2490

				0.6304	***0.5665***
Intercept	-5.7323	-5.3295	-4.4639	-5.1678	-4.2884
	(0.2257)	(0.2867)	(0.3241)	(0.3229)	(0.3461)
	-25.4033	-18.5909	-13.7745	-16.0052	-12.3905
	0.0032	***0.0048***	***0.0115***	***0.0057***	***0.0137***
Observations	396	396	396	396	396
Number of groups	22	22	22	22	22
Log Likelihood	-2113	-2110	-2103	-2109	-2101
df	3	4	7	8	11
Chi Square	447.1	459.4	504.7	464.4	513.3
BIC	4,261.79	4,262.37	4,266.50	4,283.89	4,286.16

Note. For each model, each predictor, table shows b-weight, (standard error of b-weight in parentheses), *t value*, and ***Incidence Rate Ratio (IRR).*** Exposure variable not shown.

Residual Analysis of Models

We reviewed the histogram of Anscombe residuals for Model C of Table 3. Anscombe residuals were appropriate because they standardize outcome measures (in this case stop counts) that are transformed to rates in a negative binomial model. Although the histogram of residuals displays a normal distribution (Figure 5), a box and whisker plot analysis (not shown) indicated the presence of extreme outliers that may have undue influence on model estimates.

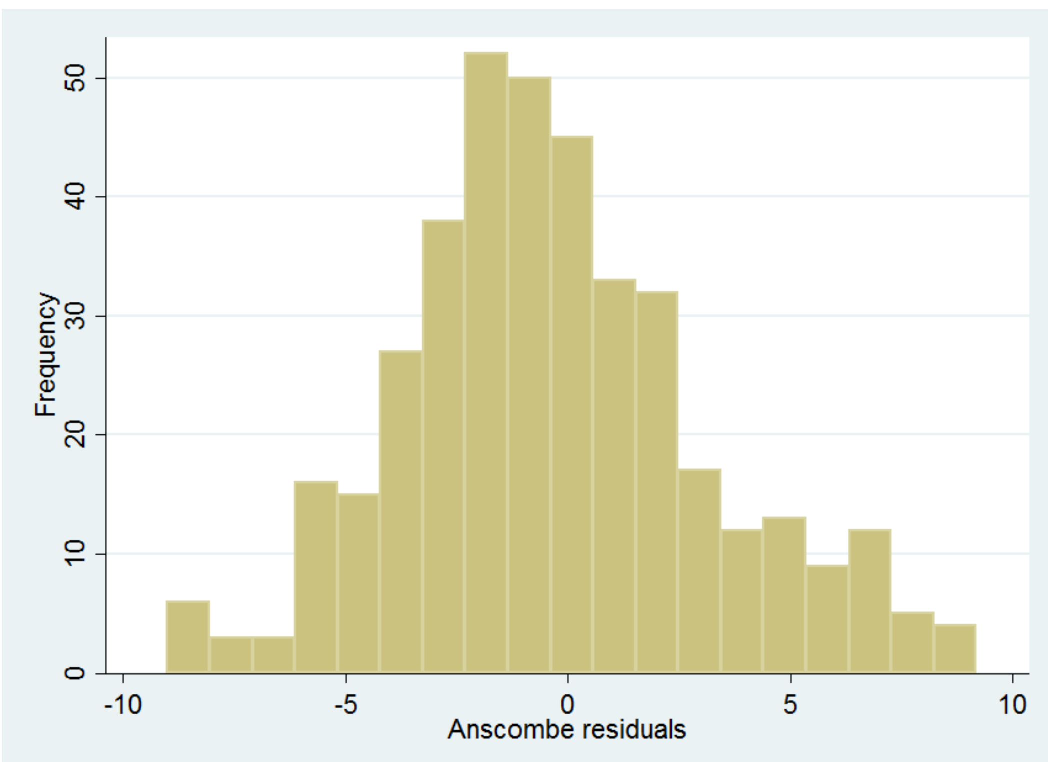


Figure 5: Standardized Residuals of Model C, Violent Arrest Exposure Variable

Translating into Predicted Stop Counts

The presence of extreme outliers in the Anscombe residuals of Model C merited additional post-estimation investigation. First, we examined the relationship between Model C's predicted mean stop rates and Anscombe residuals, considering the regression assumption of evenly distributed error terms across the predicted values. The plot of those values shown in Figure 6 does not validate that assumption.

Additional implications can be drawn from the pattern of predicted mean to residual values of Figure 6. First, Model C is more likely to underpredict when considering low stop counts. This is evidenced by the clustering of values above 5 on the residuals for smaller predicted mean values. Second, at higher stop counts, however, the model is much more likely to overpredict. This is shown by the number of residuals concentrated below -5 at higher predicted values.

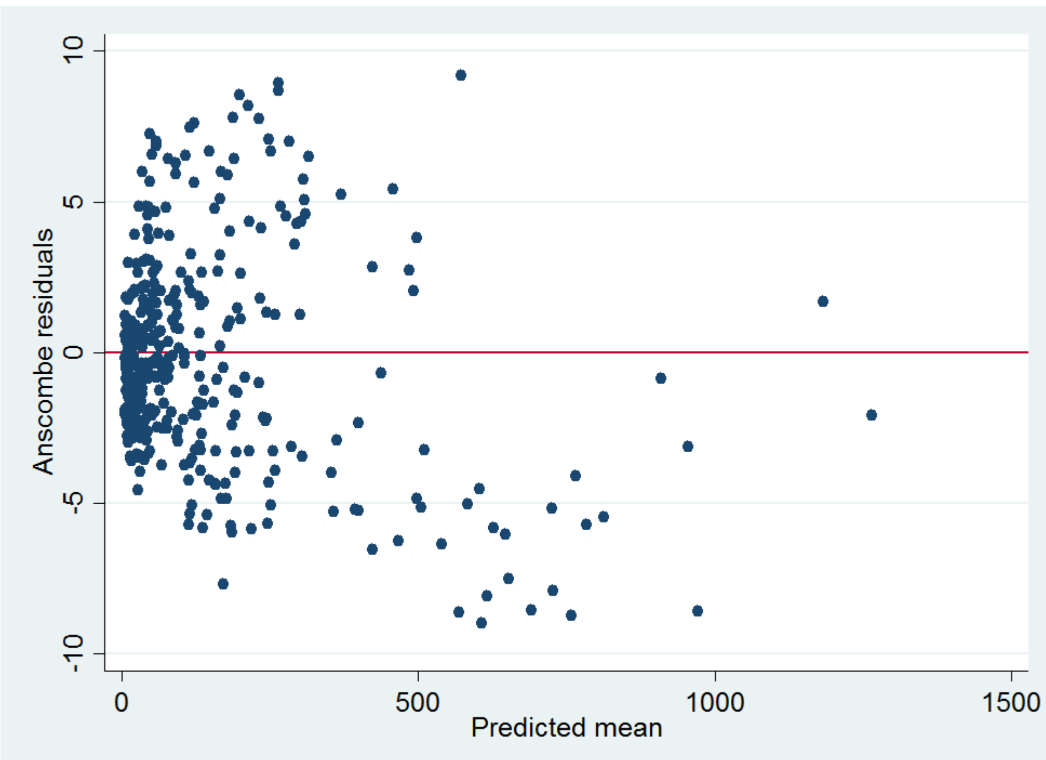


Figure 6: Predicted Stop Counts and Standardized Model C Residuals: Violent Arrest Exposure Variable

We also reviewed observed and predicted values by way of Locally Weighted Scatterplot Smoothing (LOWESS). The LOWESS function indicates the changing relationship between observed and predicted values, at different ethnoracial monthly stop count values.

Each dot represents one ethnoracial-specific district monthly stop count (Figure 7). The trendline indicates the most appropriate non-parametric trend, locally weighted, of the data.

The bend in that line suggests that Model C begins to overpredict predicted mean values at about 300 stops per arrest. The run-over-rise of the trendline, that is its movement to the right relative to its vertical movement, starts to accelerate at that point. So, for example, at a predicted value of 500 (horizontal axis), the predicted rate of 500 substantially exceeds the corresponding observed value of around 400 (vertical axis) for the same point on the trendline. Thus, predicted scores above 300 are substantially less likely than lower predicted scores to be true representations of observed ethnoracial-specific monthly stop counts.

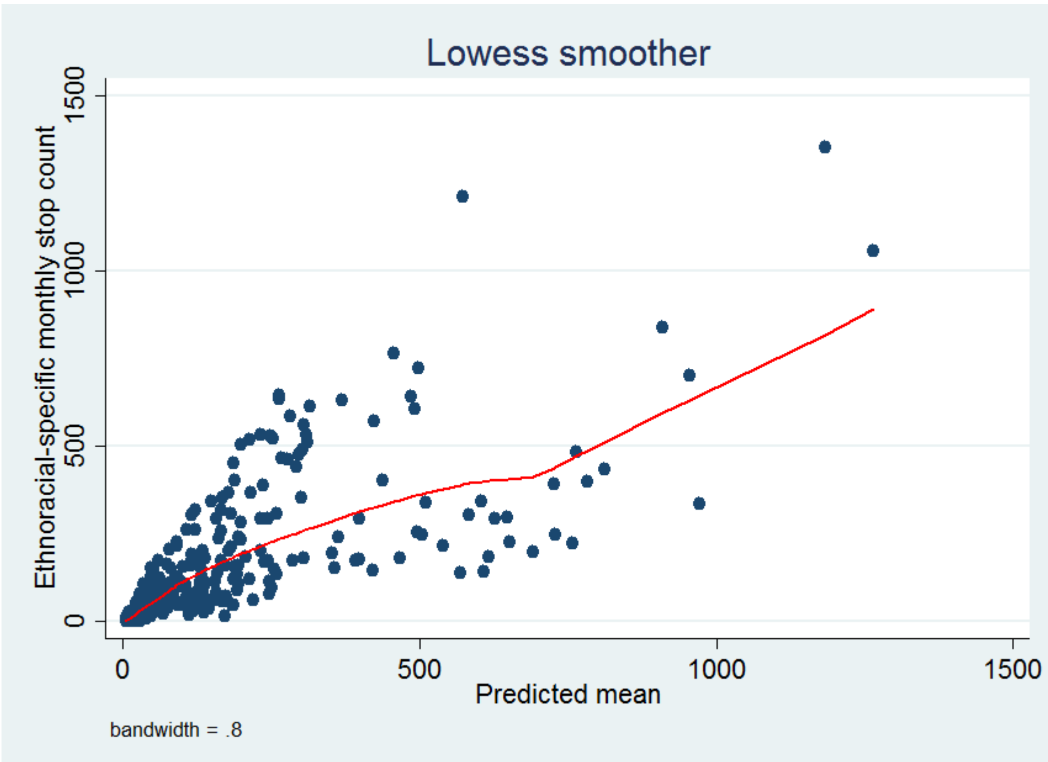


Figure 7: LOWESS Plot of Predicted to Observed Stop Counts

Model Fit Diagnostics

Model fit, or goodness-of-fit, refers to the extent to which a given set of predictors accounts for variation within the dependent variable. When multiple models are used to explain a set of observations, the model fit measures help to determine which model best accounts for said variation. To that aim, we reported Akaike Information Criterion (AIC) and Bayesian Information Criterion (BIC) model fit measures for all unconditional and conditional models. For both the AIC and BIC, lower values indicate better goodness-of-fit.

The ANOVA model for stop counts using spatial Empirical Bayesian smoothed violent arrests yielded a BIC value of 4,120.708 (APPENDIX Y). That value represented a baseline measure against which BIC values for conditional models were compared. That said, the addition of predictors did improve the prediction of stop counts. For example, the inclusion of ethnoracial predictors (non-Hispanic Black and Hispanic) in Table 3, Model A decreased the BIC value by about 26 to 4094.76. Any change greater than 10 is “very strong” evidence of improved model fit controlling for additional model complexity (Raftery 1995). Controlling for the linear effect of time, however, did not improve the predictive ability of the model. Model B shows that the addition of time increased the BIC value by less than 1—a change that was seen as worsening model fit, upon Model A (Raftery 1995). Model C added in controls for percent Black, socioeconomic status, and residential stability. The addition of those measures resulted in *poorer* model fit than Models B and A (BIC=4,104.375), but still a very strong improvement upon the ANOVA model. Models D and E, which substituted monthly time dummies for the linear monthly trend, performed the worst, with BIC values of 4,113.96 and 4,123.386, respectively.

Patterns of model goodness-of-fit using total arrests and age-weighted population were contrary to that of the violent arrest models. For example, the ANOVA BIC value for total arrests was 3,638.63. Interestingly, the additional correlates in conditional Models A, B, and C did not greatly improve model fit (Table 4). In fact, among the three models, only B demonstrated weak evidence of improved fit while controlling for model complexity, relative to the ANOVA. But, models predicting stop counts that used monthly dummy variables (Models D and E) outperformed those that did not consider time, or those that controlled for the linear temporal trend (Models A-C).

The ANOVA of stops using age-weighted population as the denominator yielded a BIC value of 4,563.50. All conditional models had BIC values that provided very strong evidence of improved model fit (Table 5). Models A and B performed the best, with BIC values of 4,262; while Model E was the worst fitting of the group Models B-E, with a BIC value of 4,286.16.

Discussion

The purpose of this study was to describe the frequency of ethnoracial-specific stop counts from July 1 through December 31, 2016, which captures the second reporting period. In accordance with the trends of Period 1, stop rates continued to decrease in the second reporting period as well. Furthermore, the downward trend in stop rates was evident when calculated per 1,000 residents, per 100 violent arrests of the previous month, or per 100 total arrests of the previous month. And, within ethnoracial groups, decreases in stop counts were evident as well when comparing the average monthly rates of the current period to a segment of the first reporting period. For example, over the six-month period stop rates per population decreased by 79% for Hispanic Whites, 82% for non-Hispanic Whites, and by 81% for non-Hispanic Blacks, compared to the average monthly rates of July through December 2015.

We formed inferences from our descriptive findings through the interpretation of several mixed effects negative binomial models. Using such models, we regressed ethnoracial-specific stop counts against ethnoracial, temporal, and demographic structural indicators. Counts were transformed to rates using three denominators: spatial Empirical Bayesian smoothed violent arrests of the month prior, total arrests of the month prior, and age-then-gender-weighted total population.

As in the first reporting period, our findings varied by denominator. The specific size of the differential between non-Hispanic White detainee stop rates and non-Hispanic Black detainee stop rates depended on the denominator. The differential in stop rates was anywhere from expected rate differences of 14 percent higher to expected rate differences ten times higher.

The fact that the non-Hispanic Black vs. White differential varied depending on the denominator is completely understandable because the stop counts for each group are being benchmarked against different exposure measures to construct the stop rates. It is a very different matter, *conceptually as well as operationally*, to benchmark stops against ethnoracial-specific violent arrests vs. ethnoracial-specific total arrests vs. age-then-gender-weighted population. *The different rates constructed, and therefore the differences in rates between non-Hispanic Blacks vs. non-Hispanic Whites,* ***mean different things****.*

At the same time, even though there are differences across denominators in the size of the Black/White rate differentials, ***in all cases, with all three denominators, the Black/White rate differentials always prove statistically significant*** *suggesting in that sense that the disparities are consistently observed.* ***The denominator chosen for constructing the stop rate, even though three markedly different exposure variables are used as the denominators for the three different rates, does NOT affect the conclusion that Black non-Hispanic stop rates are significantly higher than White non-Hispanic stop rates.*** Across all three denominators, the t-statistic associated with the Black/White rate differential was never smaller than about 2.7. So, the Black/White difference is ***always*** significant at $p < .01$ in ***all*** the models. For *all* the models the chances that the Black/White differentials observed arise just from chance variation is always less than one in a hundred.

Simply put, if the question is, does the choice of denominator for constructing stop rates affect the conclusion that a statistically significant disparity exists between non-Hispanic White and non-Hispanic Black stop rates, ***the answer is an emphatic no.*** The statistically significant disparity is always there in all the models. So even though three different denominators make three conceptually very different stop rates, there is always significant disparity between these two key groups in the rate at which they generate stops.

That equivalence of the denominators aside, if forced to pick a favorite, our preference remains towards models employing one- month-lagged violent arrests. The age-weighted population models are not ideal because the population measure is not ethnoracial-specific. As such, the calculation of stop rates in these models suffers from a large degree of misspecification due to race being central to the numerator, but not the denominator. Of course, that denominator could be constructed along ethnoracial/age/gender-specific lines. But even if we were to do so, considerable slippage remains between the residential population in a district and the population of persons on the street or in cars in a district who are of potential interest to patrolling officers.

The total arrest measure has an advantage in that it is ethnoracial-specific. But it is characterized by substantial officer discretion in arrest decision making, considering that it is skewed numerically towards low-level offenses. Because of this, it is not preferable since our interest lies in the extent to which *less* discretionary arrest actions are associated with differentials in ethnoracial stop rates.

We do recognize that the violent arrest denominator is a less than perfect measure. Serious model limitations are noted in brief below, and in greater detail in the results section of this report. But, we do find that its limitations are less problematic than competing exposure measures, and note that findings of the violent arrest models tend to align with results from studies of other large urban areas (Gelman et al. 2007).

Before leaving this violent arrest denominator, it is important to flag a potential misunderstanding. This denominator and the resulting rate are *not* saying that the persons in a district who are arrested for a violent crime in a month will be the same group of people who are stopped by police in that district the next month. *The denominator, and the numerator, and therefore each specific rate, are all properties of the* ***locale itself.*** These are ecological indicators.

In sum, ethnoracial effects remain statistically important in the prediction of stop counts. Stops of non-Hispanic Blacks significantly exceeded those of non-Hispanic Whites. This statistically important difference surfaces regardless of the type of rate constructed. On the other hand, stop counts of Hispanics and non-Hispanic whites were statistically indistinguishable. Additionally, investigatory stop counts were less likely to occur in districts that were predominately non-Hispanic Black, or of high socioeconomic status.

Limitations

Two limitations characterize our findings.

First, the most reliable models (using spatial Empirical Bayesian smoothed violent arrests as the denominator) did violate fundamental assumptions of linear regression, as demonstrated by the residual analysis. That said, our analyses from the first reporting period also suffered from the same limitation. Although we speculated that our spatial smoothing approach would alleviate this concern, it may be that this issue is not solvable, and is relative to the unique properties of police stop data.

Second, although our use of multilevel models was theoretically grounded in the nesting of time within police districts, our analyses were limited by the low number of Chicago police districts (Bryan and Jenkins 2016; Schmidt-Catran and Fairbrother 2016). Thus, district-level impacts must be interpreted with caution. As noted in the first report, it would be preferable to model police beats as the unit of analysis, nested within districts. But, doing so could exacerbate another issue. A smaller unit of analysis would likely increase the prevalence of zero counts for the violent arrest denominator.

Conclusions

Focusing on our most reliable models (those using violent arrests as the denominator), sizable disparities in stop rates existed between stops of non-Hispanic Whites and non-Hispanic Blacks. Stops counts of the latter group exceeded the former by 82%. Yet, this disparity is smaller, descriptively speaking, than the 125% difference identified by models of a comparable section of the first reporting period. While the Black versus White stop count effect persisted across models, regardless of the denominator used, the Hispanic versus White stop count effect emerged only in the total arrest and age-weighted population models.

Second, temporal and racial composition effects are consistent across models, regardless of denominator type. Thus, the robustness of these effects suggests that they are reliable and noteworthy. We are fairly certain that stop counts were decreasing over time, and that predominantly Black districts reported fewer stop counts, controlling for other covariates.

1. Open ArcMap
2. Add the beats/district shapefile as a layer
3. Add the census block group shapefile as a layer. (This file does not need to have the key ACS variables we're using. A boundary file with no variables is fine.)
4. Make sure that both the beat/district and block group shapefiles have the same projection: NAD_1983_StatePlane_Illinois_East_FIPS_1201_Feet.
5. Open the block group shapefile's attribute table. **Add Field** [double]. Name it 'Area'
    a. Right click on the field then **Calculate Geometry**. Units = Square Miles US
6. **ArcToolbox > Intersect**
    a. In the Features box add the beat/district layer first, then the census block group layer
    b. Click **OK**
7. Open the *new* beat/district attribute table. Notice 1.) the increase in total number of cases (because the beat/district file has been intersected) and 2.) A field indicating the GEOIDs of the relative block groups.
8. Now, you need to calculate the area of intersected features. Using the *new* beat/district attribute table: **Add Field** [double]. Name it 'Newarea'
    a. Right click on the 'Newarea' field. **Calculate Geometry**. Units = Square Miles US
9. At this point, you can calculate the proportion of each block group that falls within a given tract. **Add Field** [double]. Name it 'AreaP'
    a. Using the **Field Calculator** 'AreaP' = [Newarea]/[Area]
    b. Click **OK**
10. Save the shapefile
11. Export the table as a DBF or whatever format is easiest for you to open in Stata
12. Use your GEOID to merge the ACS data to the table
13. Create interpolated variables by multiplying the 'AreaP' variable by each ACS count variable.
14. You can then use Stata's **Collapse** command to sum all count observations by GEOID.

APPENDIX B: Smoothed Versus Raw Rate Violent Arrest Distributions

In order to justify the use of smoothed rates instead of raw rates or inflated raw rates, the skewness of the distribution of observations between the inflated rates and the smoothed rates for each ethnoracial group in each month were calculated (depicted in Figures 1-36 below). In total, 83.3% of the ethnoracial groups within months saw an improvement in skewness when smoothing the rates and improving overall skewness by 62.6%.

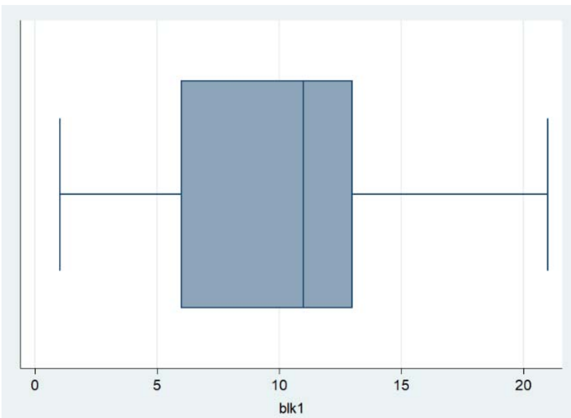


Figure 1: Distribution of black violent arrests +1 in June.

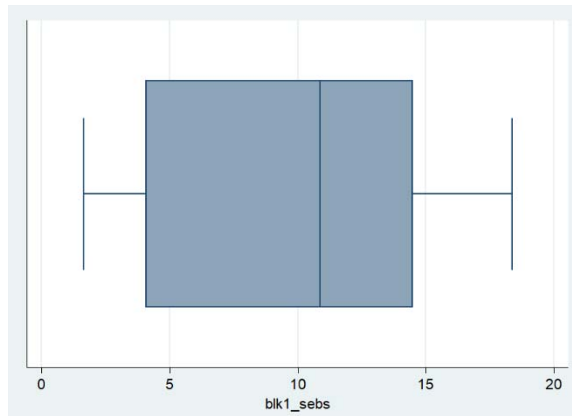


Figure 2: Distribution of smoothed black violent arrests +1 in June. Skewness increased by 114.42%.

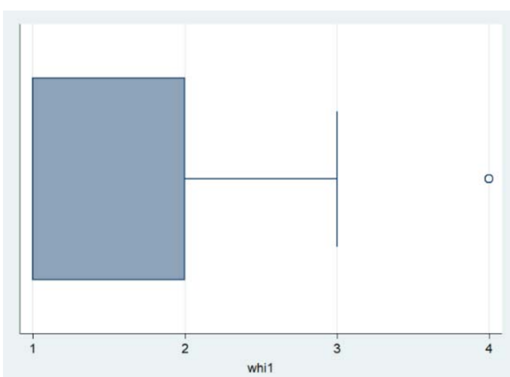


Figure 5: Distribution of white violent arrests +1 in June.

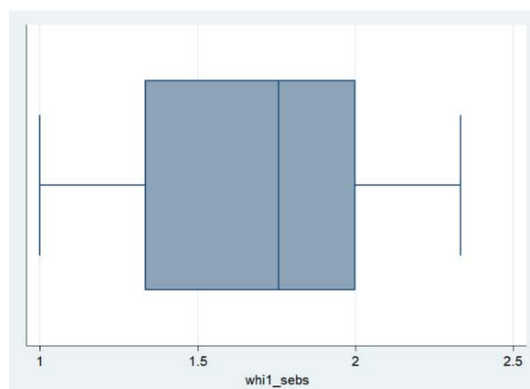


Figure 4: Distribution of smoothed white violent arrests +1 in June. Skewness decreased by 84.03%

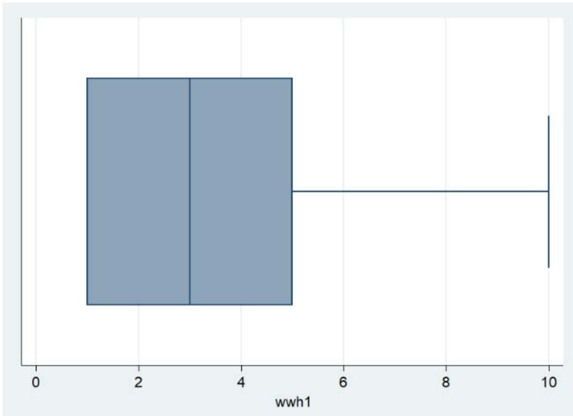


Figure 5: Distribution of Hispanic violent arrests +1 in June

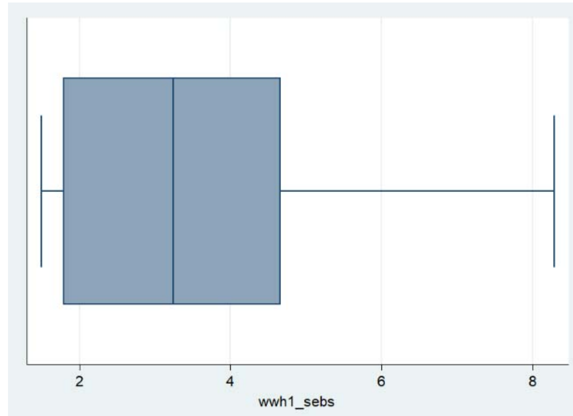


Figure 6: Distribution of smoothed Hispanic violent arrests +1 in June. Skewness decreased by 2.09%

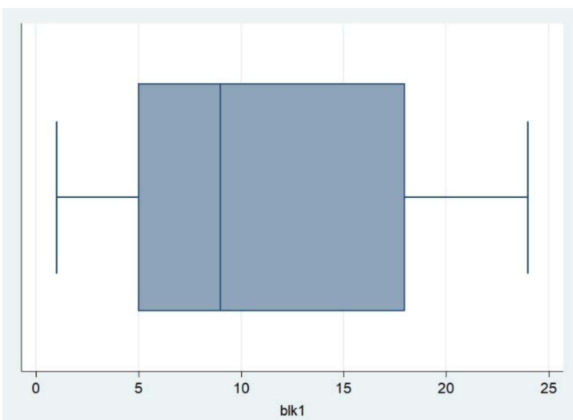


Figure 7: Distribution of black violent arrests +1 in July.

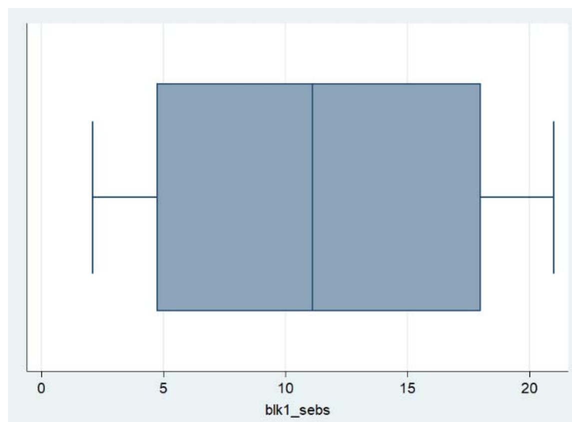


Figure 8: Distribution of smoothed black violent arrests +1 in July. Skewness decreased by 61.78%.

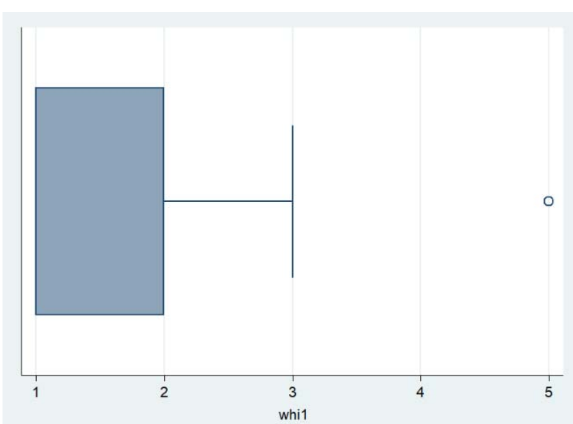


Figure 9: Distribution of white violent arrests +1 in July.

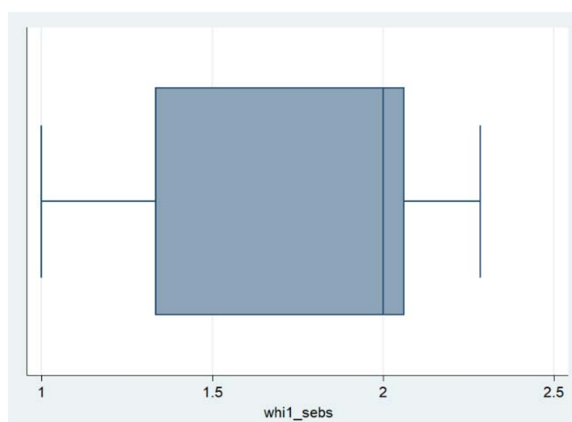


Figure 10: Distribution of smoothed white violent arrests +1 in July. Skewness decreased by 67.69%.

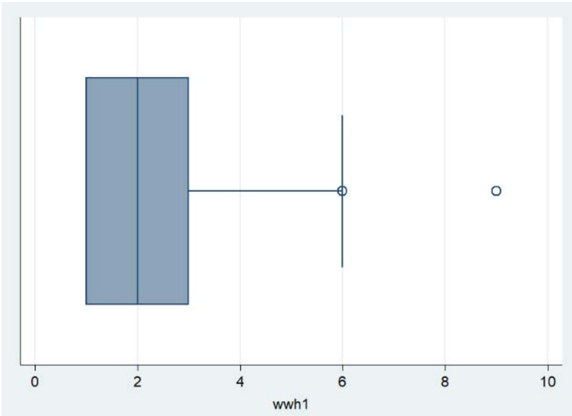


Figure 11 : Distribution of Hispanic violent arrests +1 in July.

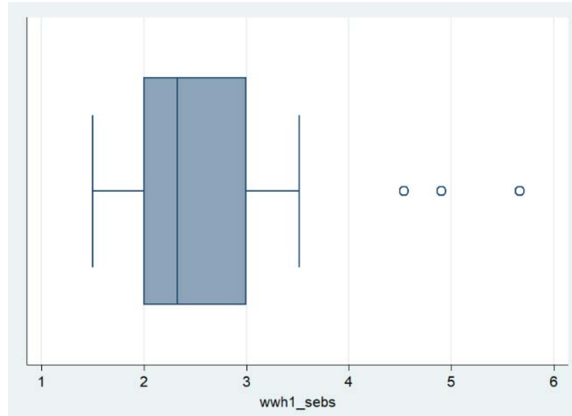


Figure 12: Distribution of smoothed Hispanic violent arrests +1 in July. Skewness decreased by 15%.

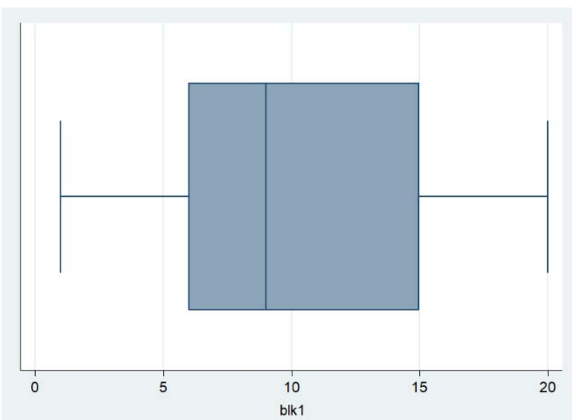


Figure 13 : Distribution of black violent arrests +1 in Aug.

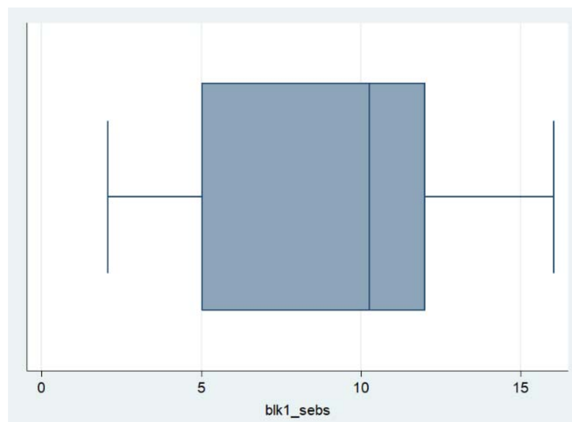


Figure 14: Distribution of smoothed black violent arrests +1 in Aug. Skewness decreased by 44.81%.

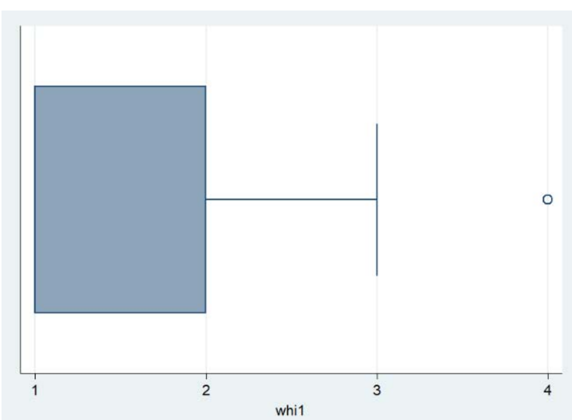


Figure 15: Distribution of white violent arrests +1 in Aug.

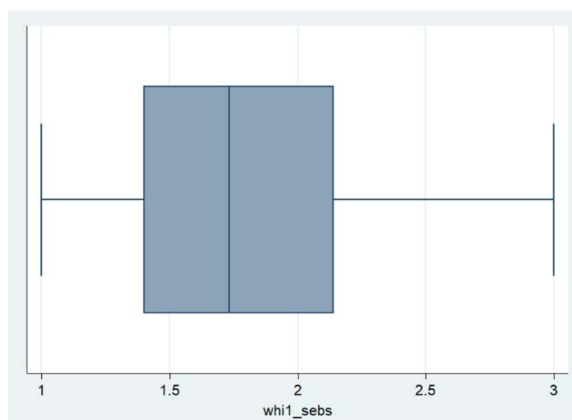


Figure 16: Distribution of smoothed white violent arrests +1 in Aug. Skewness decreased by 49.23%.

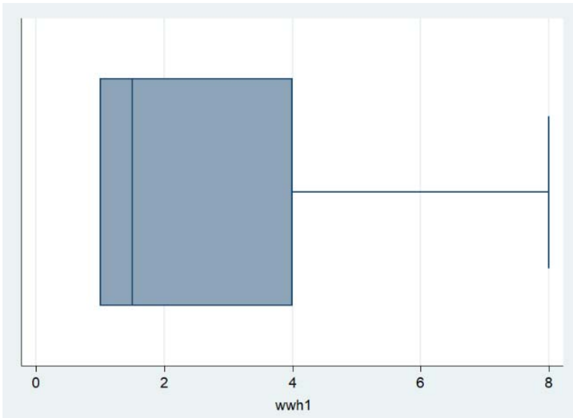


Figure 17: Distribution of Hispanic violent arrests +1 in Aug.

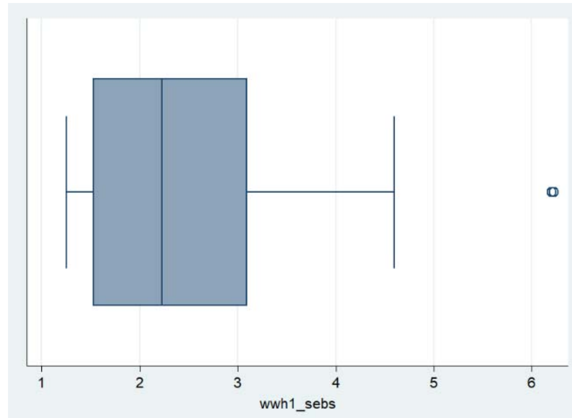


Figure 18: Distribution of smoothed Hispanic violent arrests +1 in Aug. Skewness decreased by 4.96%.

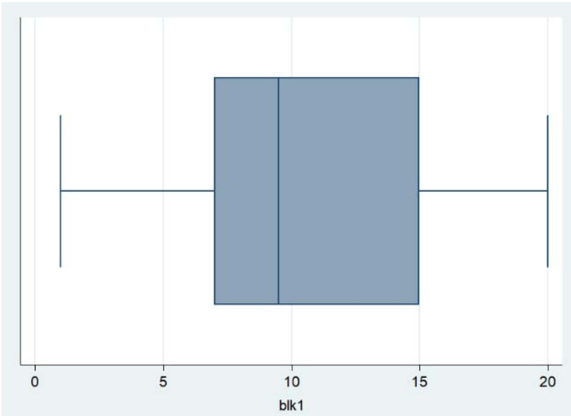


Figure 19: Distribution of black violent arrests +1 in Sept.

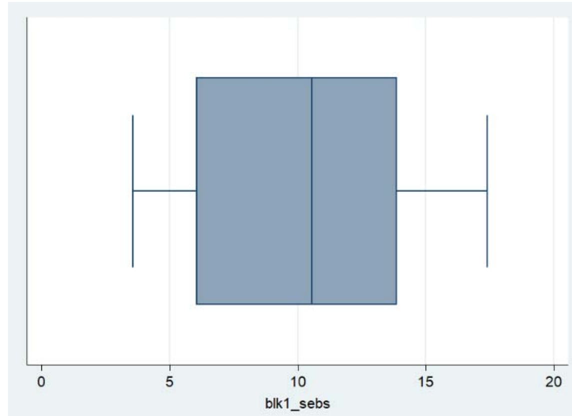


Figure 20: Distribution of smoothed black violent arrests +1 in Sept. Skewness decreased by 74.58%.

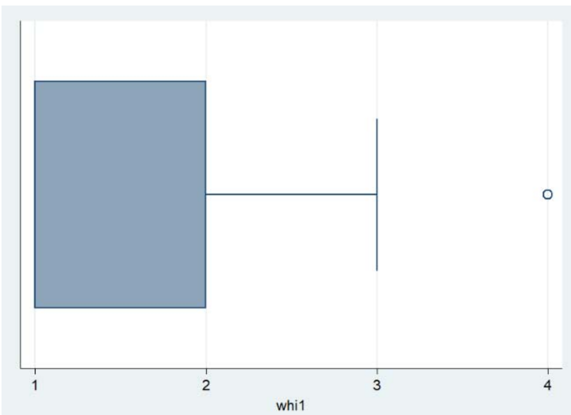


Figure 21: Distribution of white violent arrests +1 in Sept.

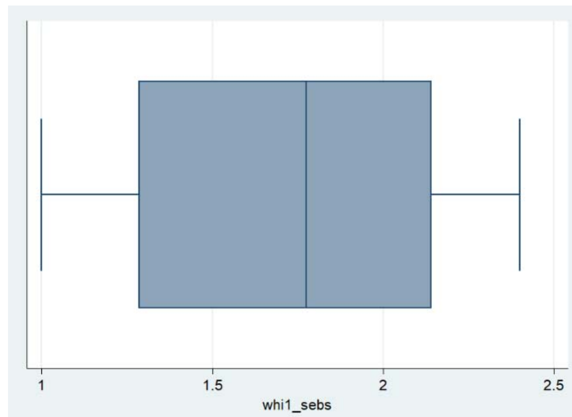


Figure 22: Distribution of smoothed white violent arrests +1 in Sept. Skewness decreased by 73.84%.

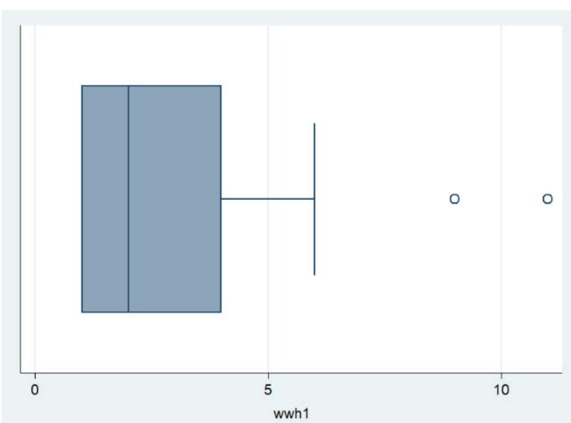


Figure 23: Distribution of Hispanic violent arrests +1 in Nov.

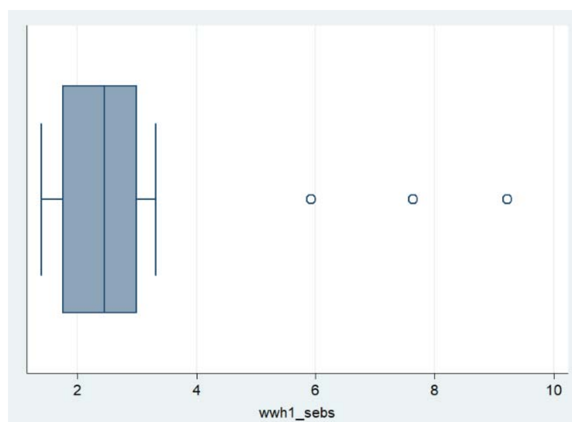


Figure 24: Distribution of smoothed Hispanic violent arrests +1 in Sept. Skewness increased 22.69%.

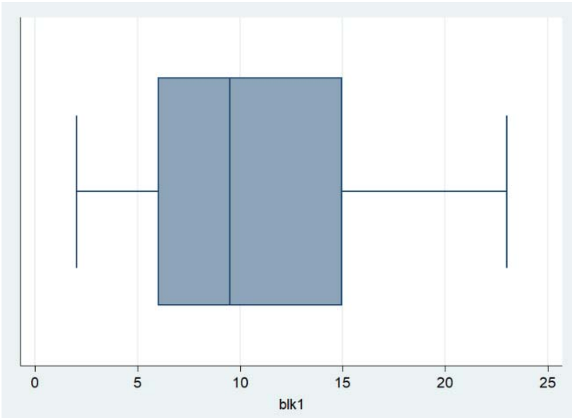


Figure 25: Distribution of black violent arrests +1 in Oct.

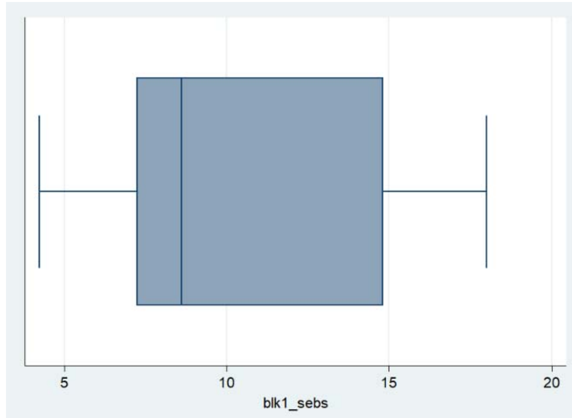


Figure 26: Distribution of smoothed black violent arrests +1 in Oct. Skewness increased 31.76%.

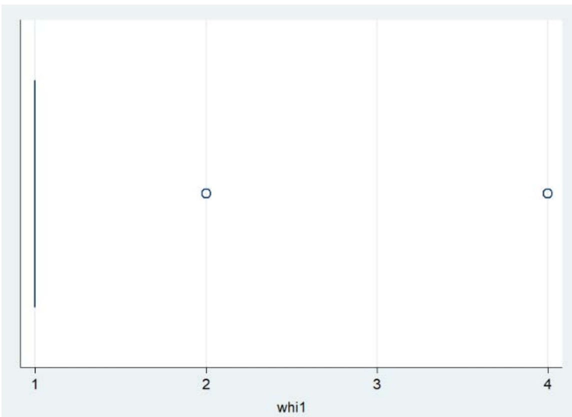


Figure 27: Distribution of white violent arrests +1 in Oct.

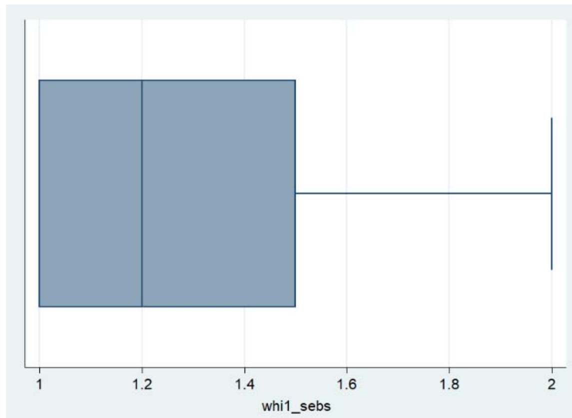


Figure 28: Distribution of smoothed white violent arrests +1 in Oct. Skewness decreased by 66.02%.

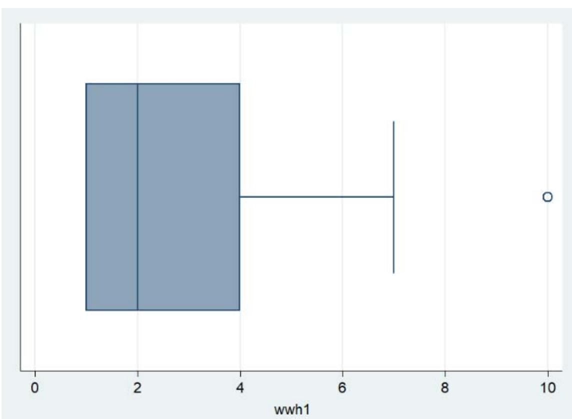


Figure 29: Distribution of Hispanic violent arrests +1 in Oct.

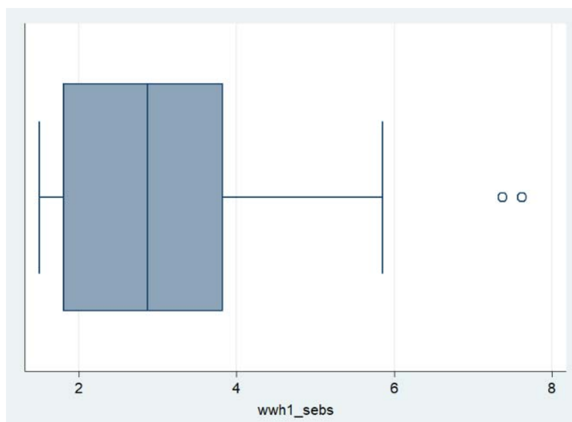


Figure 30: Distribution of smoothed Hispanic violent arrests +1 in Oct. Skewness decreased by 8.14%.

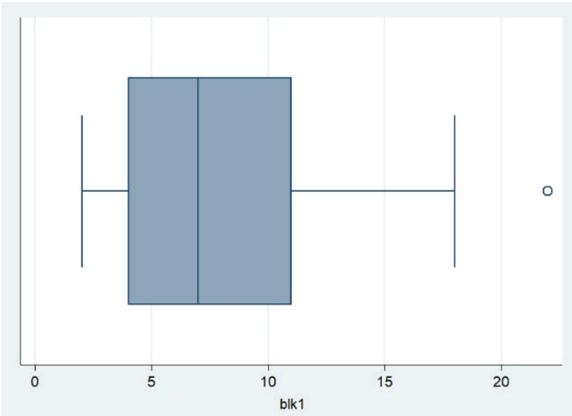


Figure 31: Distribution of black violent arrests +1 in Nov.

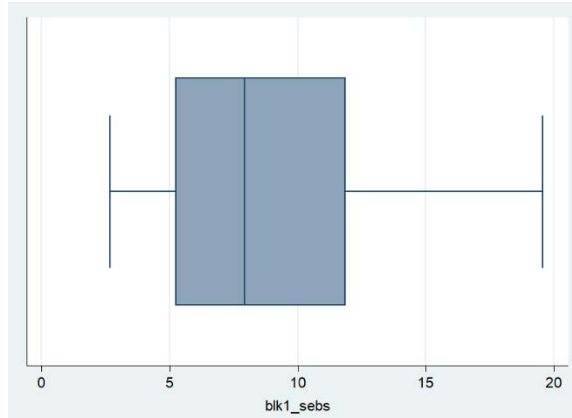


Figure 32: Distribution of smoothed black violent arrests +1 in Nov. Skewness decreased by 16.28%.

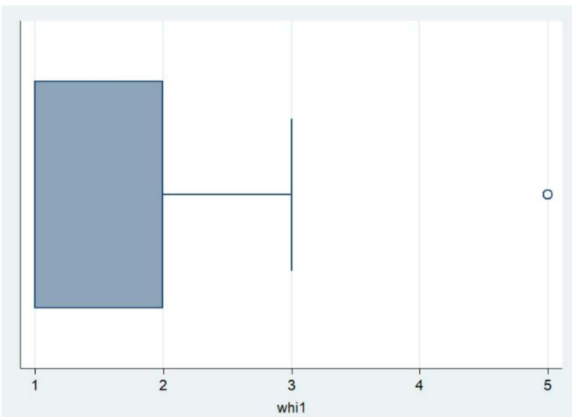


Figure 33: Distribution of white violent arrests +1 in Nov.

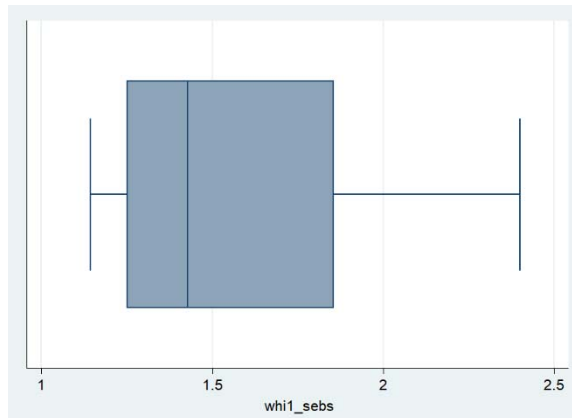


Figure 34: Distribution of smoothed white violent arrests +1 in Nov. Skewness decreased by 63.72%.

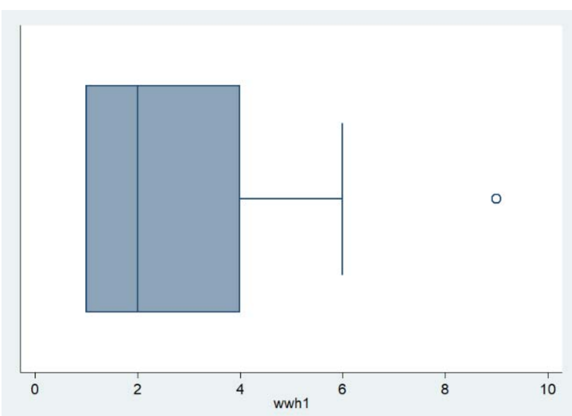


Figure 35: Distribution of Hispanic violent arrests +1 in Nov.

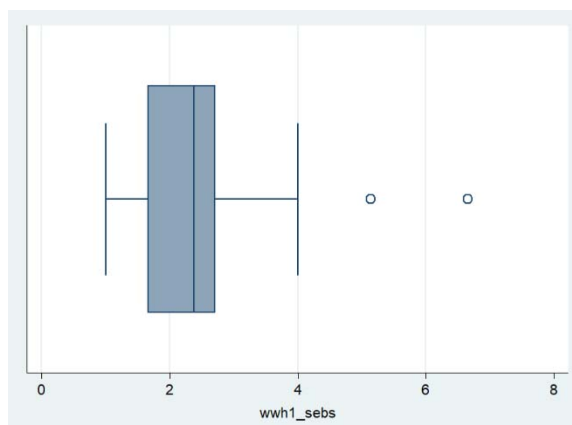
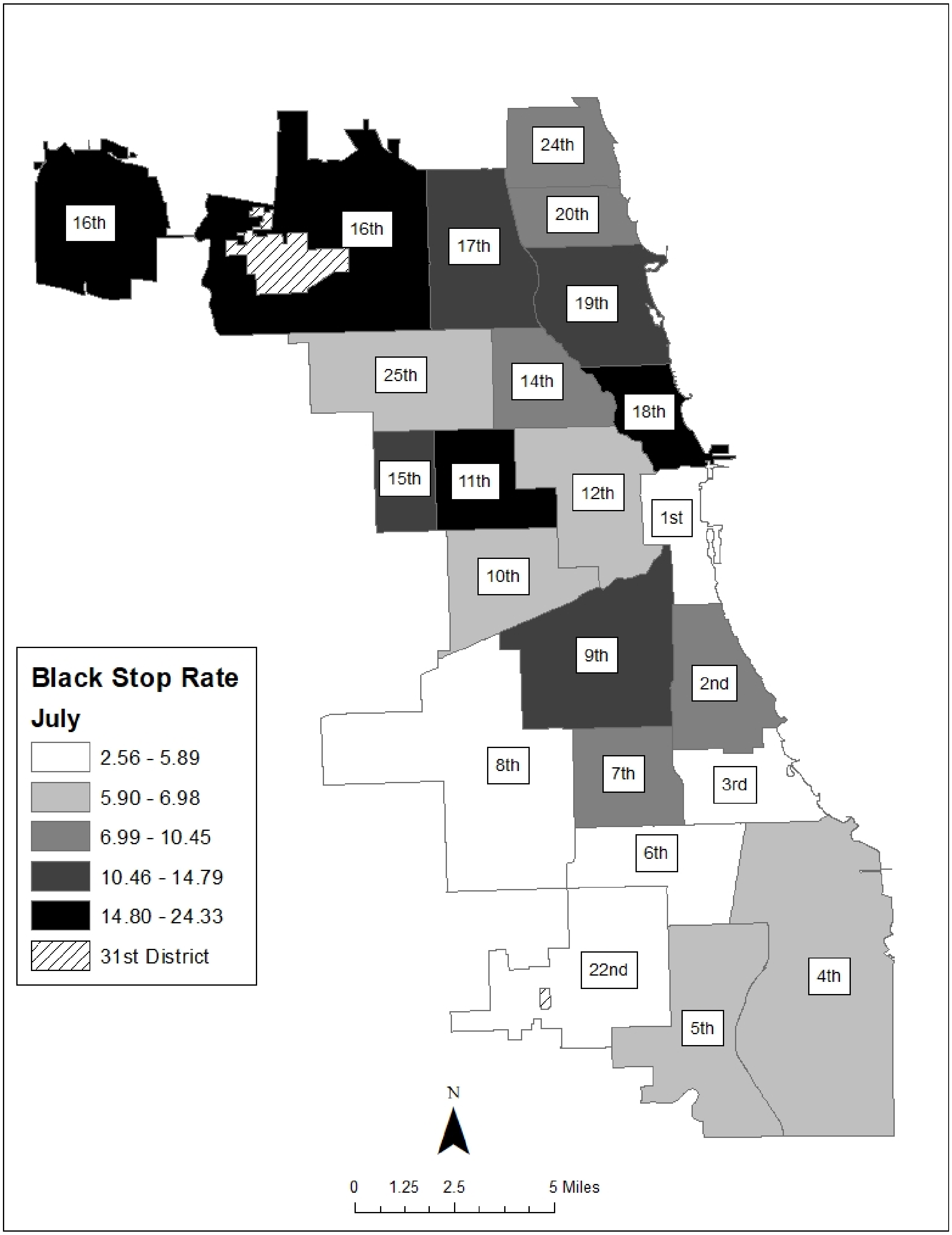


Figure 36: Distribution of smoothed Hispanic violent arrests +1 in Nov. Skewness decreased by 6.64%.

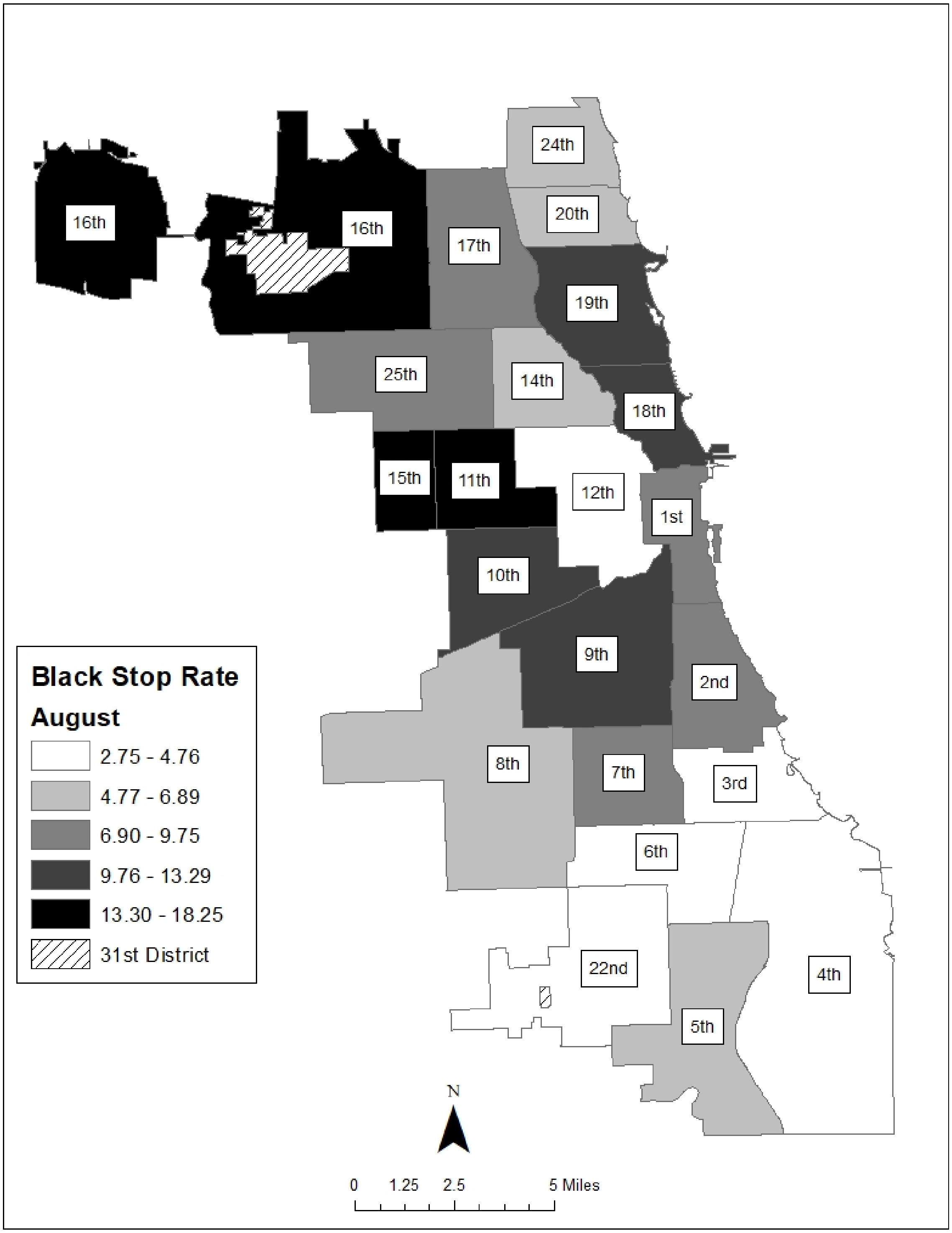
Skewness (absolute values)									
Arrest Month	Black	Black smoothed	Change	White	White smoothed	Change	Hispanic	Hispanic smoothed	Change
June	0.104	0.223	-114.42%	1.265	0.202	84.03%	0.957	0.937	2.09%
August	0.314	0.12	61.78%	1.86	0.601	67.69%	1.707	1.451	15.00%
August	0.366	0.202	44.81%	1.097	0.557	49.23%	1.39	1.321	4.96%
September	0.177	0.045	74.58%	1.097	0.287	73.84%	1.631	2.001	-22.69%
October	0.507	0.668	-31.76%	3.334	1.133	66.02%	1.523	1.399	8.14%
November	0.94	0.787	16.28%	2.139	0.776	63.72%	1.656	1.546	6.64%
Total	0.445	0.275	38.20%	1.644	0.311	81.08%	1.513	1.629	-7.67%

```
***Set directory

*cd

***CD Main -> Data -> Geo -> Erg monthly rates ->

***Add to template

*** Include Label names

** Don't forget log file

gen
po_ma_adj=((n_m05+n_m5_9+n_m1014)*.02591)+((n_m1517+n_m1819)*.2002)+((n_m20+n_m21+n_
m2224)*0.20522)+(n_m2529*0.140377)+(n_m3034*0.09168)+(n_m3539*0.07055)+(n_m4044*0.05345
)+(n_m4549*0.05652)+(n_m5054*0.06174)+(n_m5559*0.05224)+((n_m6061+n_m6264)*0.02498)+((n_
m6566+n_m6769+n_m7074+n_m7579+n_m8084+n_m85pl)*0.01403)

label po_ma_adj "Adjusted male Population"


gen
po_fe_adj=((n_f05+n_f5_9+n_f1014)*0.04245)+((n_f1517+n_f1819)*0.18999)+((n_f20+n_f21+n_f2224)
*0.15625)+(n_f2529*0.12763)+(n_f3034*0.08144)+(n_f3539*0.07204)+(n_f4044*0.06914)+(n_m4549*
0.07550)+(n_f5054*0.07135)+(n_f5559*0.03955)+((n_f6061+n_f6264)*0.01590)+((n_f6566+n_f6769+n
_f7074+n_f7579+n_f8084+n_f85pl)*0.05877)

label po_fe_adj "Adjusted female population"


gen po_ma_wght=(po_ma_adj*0.860)

label po_ma_wght "Weighted Male Population"


gen po_fe_wght=(po_fe_adj*0.140)

label po_fe_wght "Weighted Female Population"


gen tot_pop_wght=(po_ma_wght+po_fe_wght)

label tot_pop_wght "Total Weighted Population"


save pop_totals_weighted, replace
```

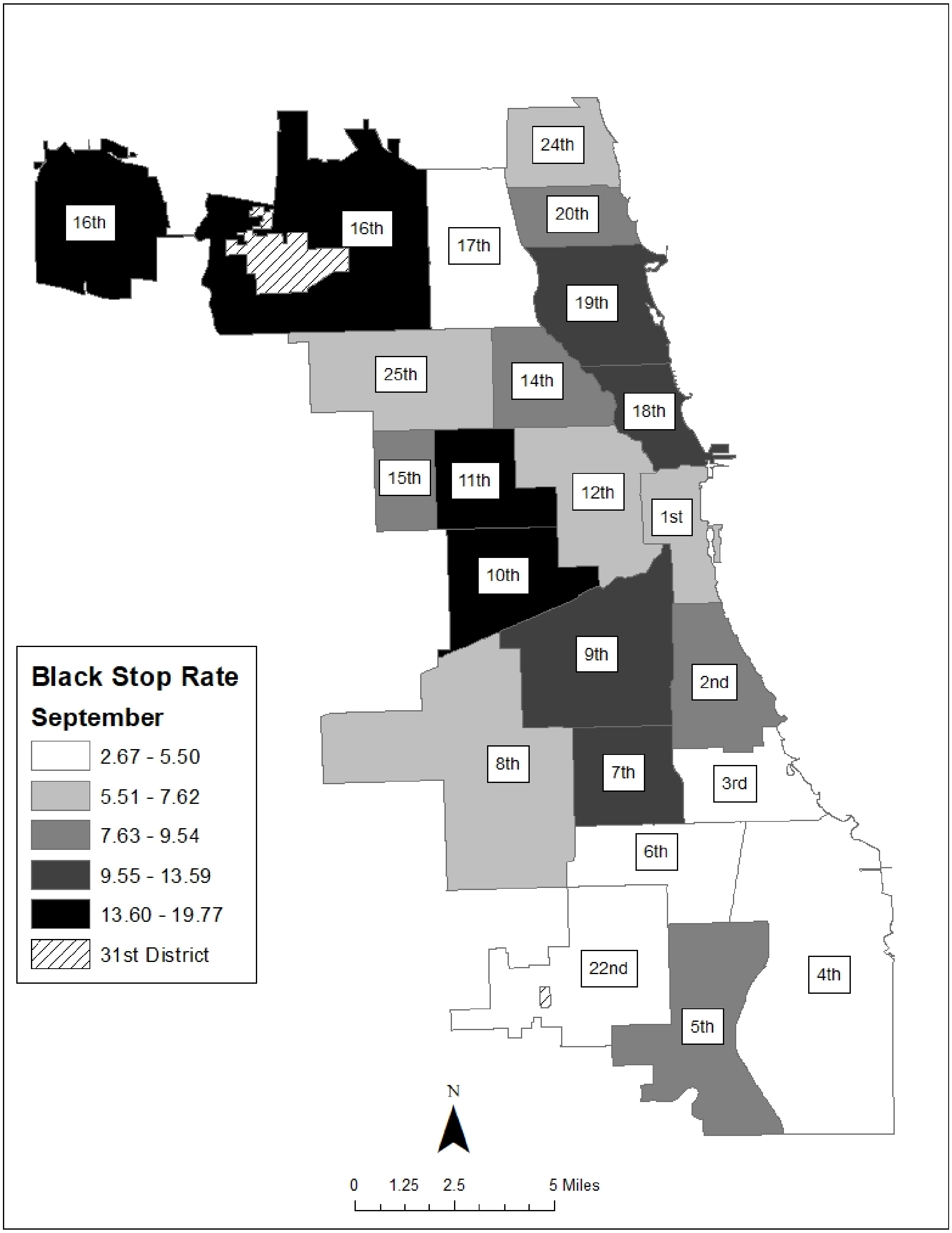
APPENDIX D: Black Stop Rate (per 1,000), July 2016



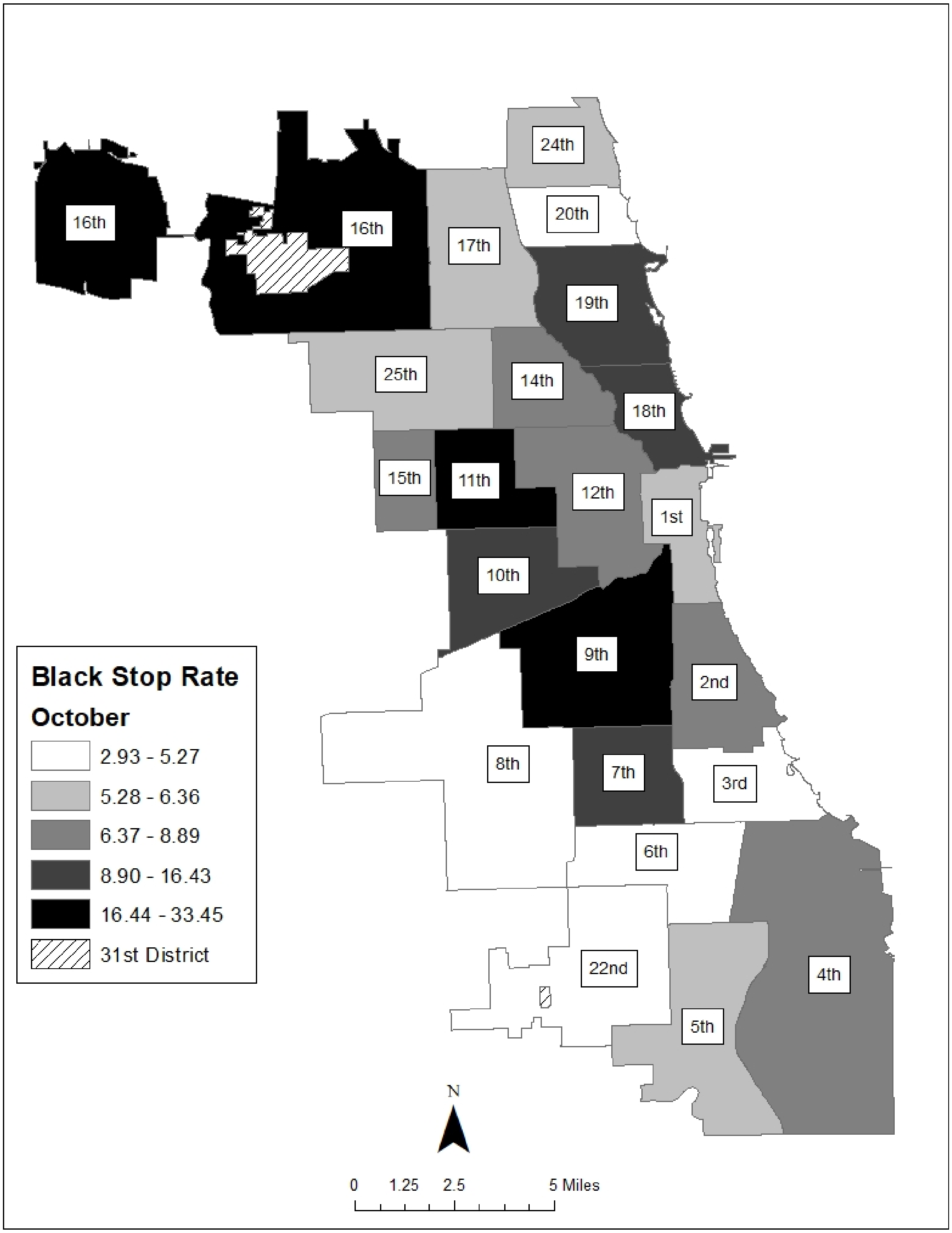
APPENDIX E: Black Stop Rate (per 1,000) August 2016



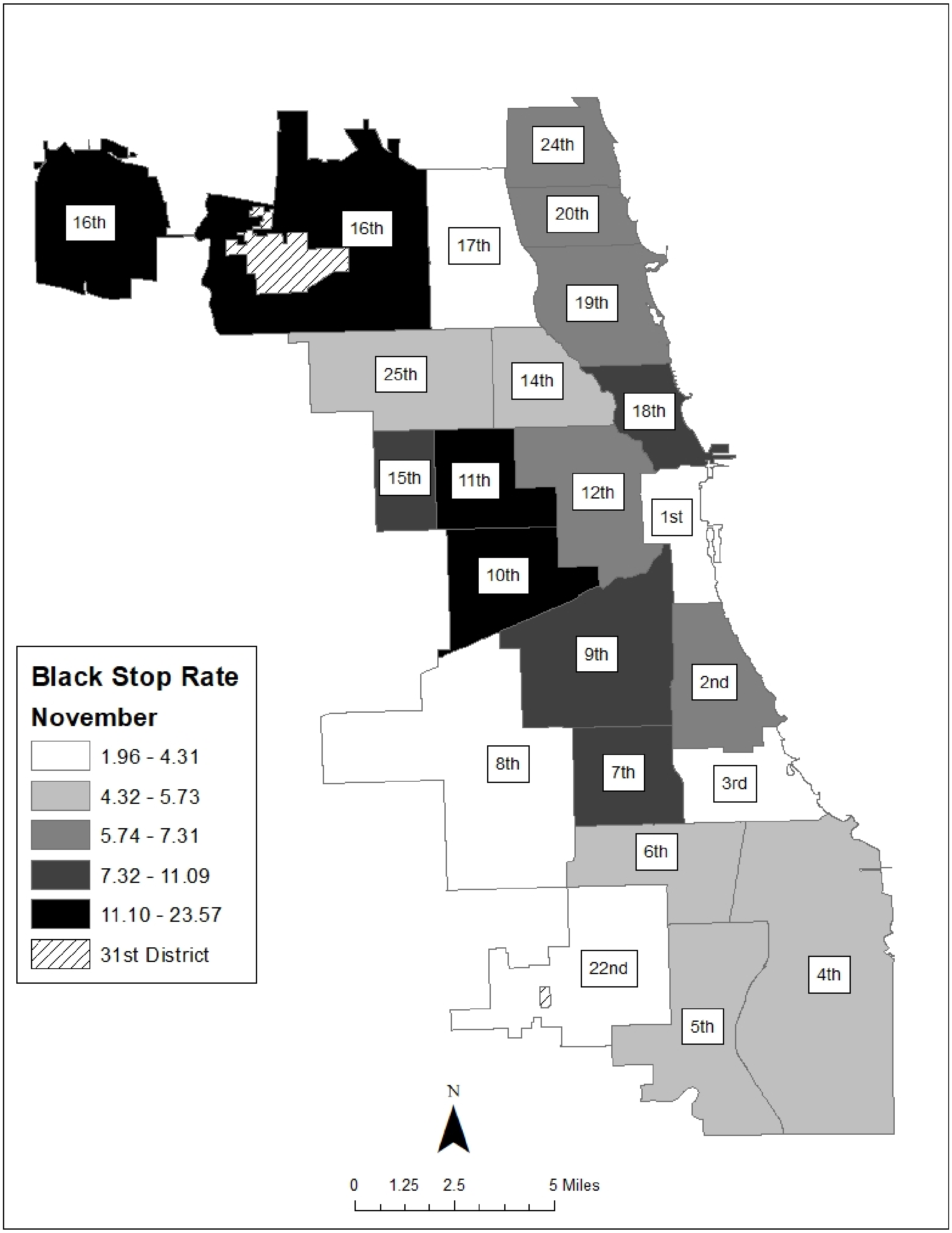
APPENDIX F: Black Stop Rate (per 1,000), September 2016



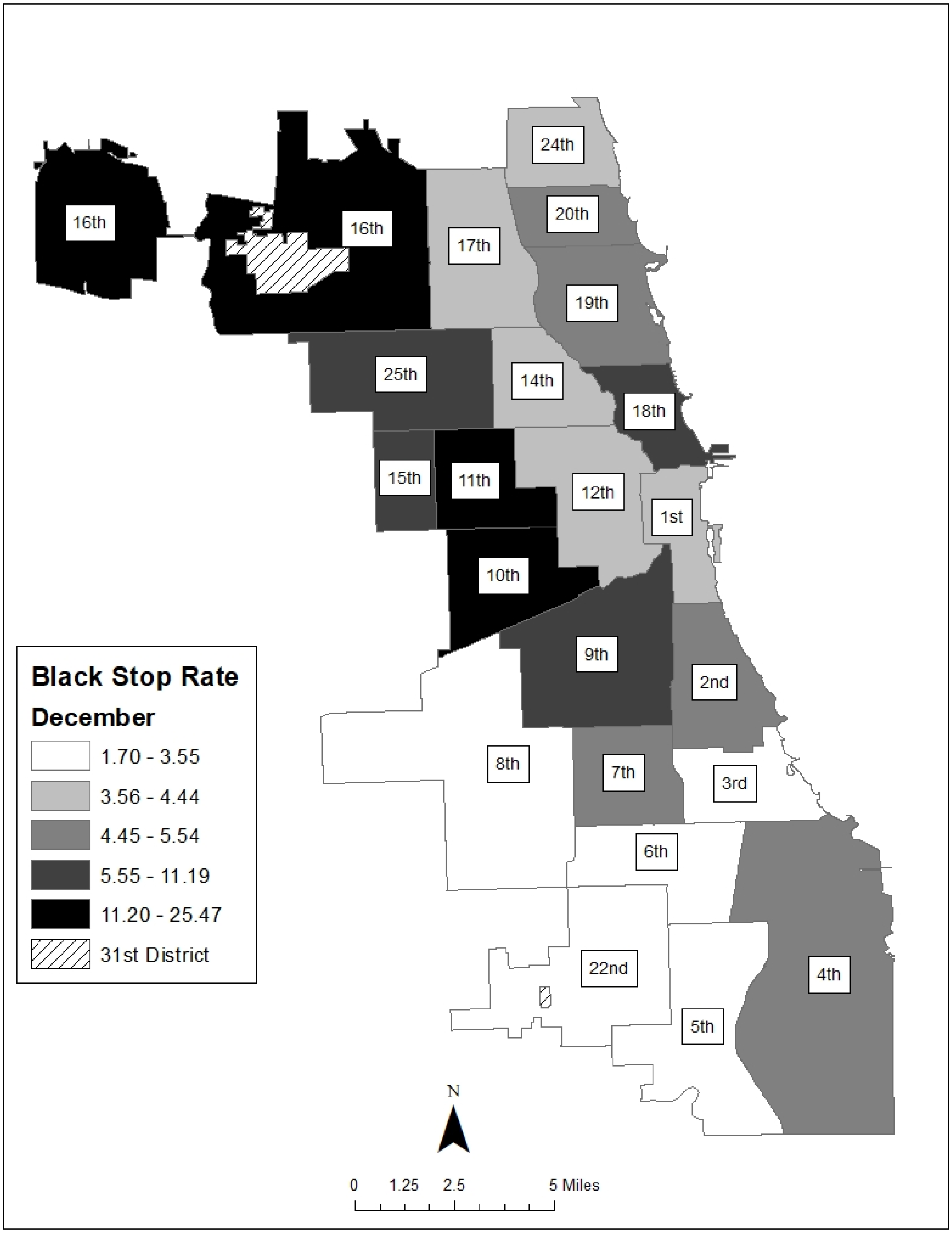
APPENDIX G: Black Stop Rate (per 1,000), October 2016



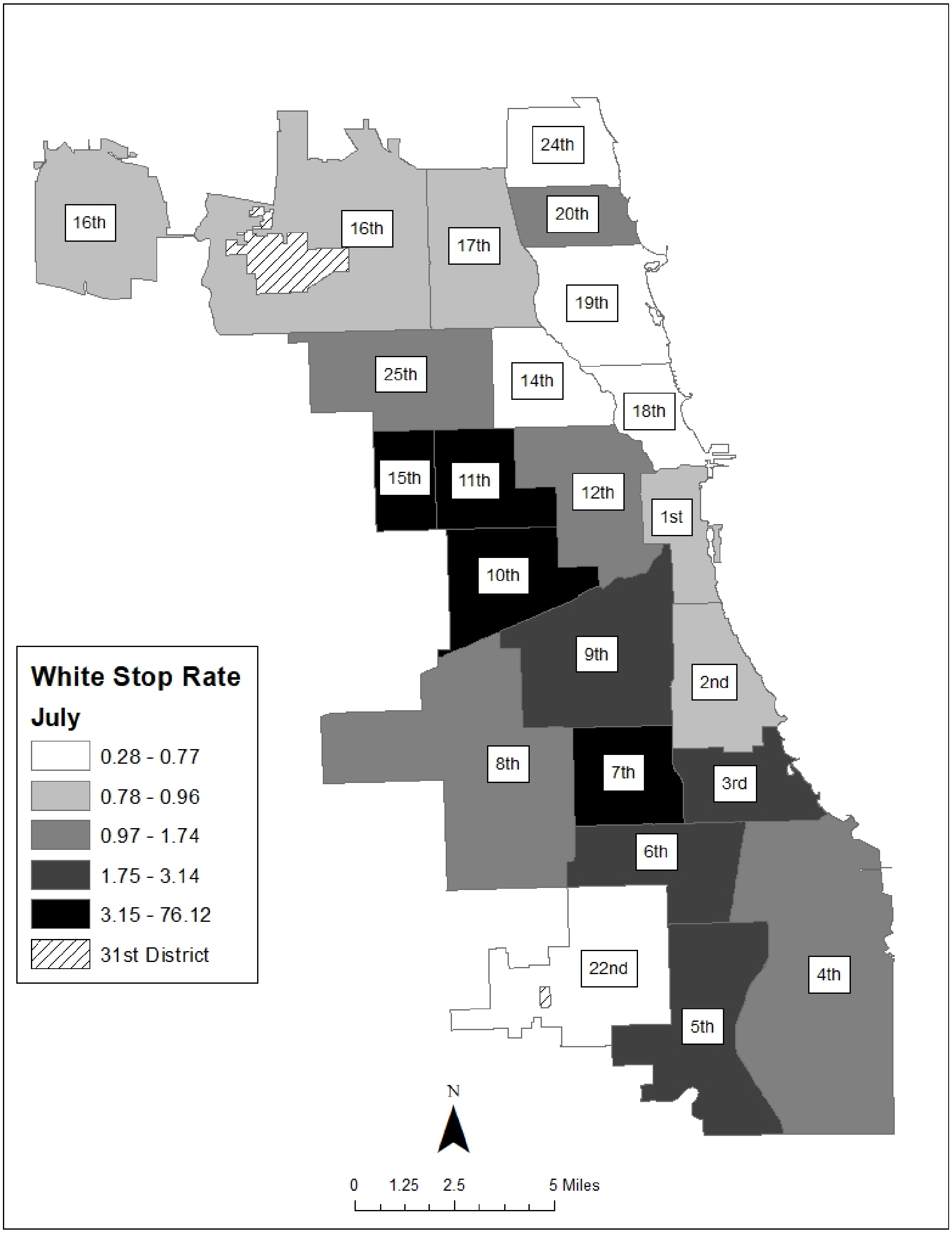
APPENDIX H: Black Stop Rate (per 1,000), November 2016



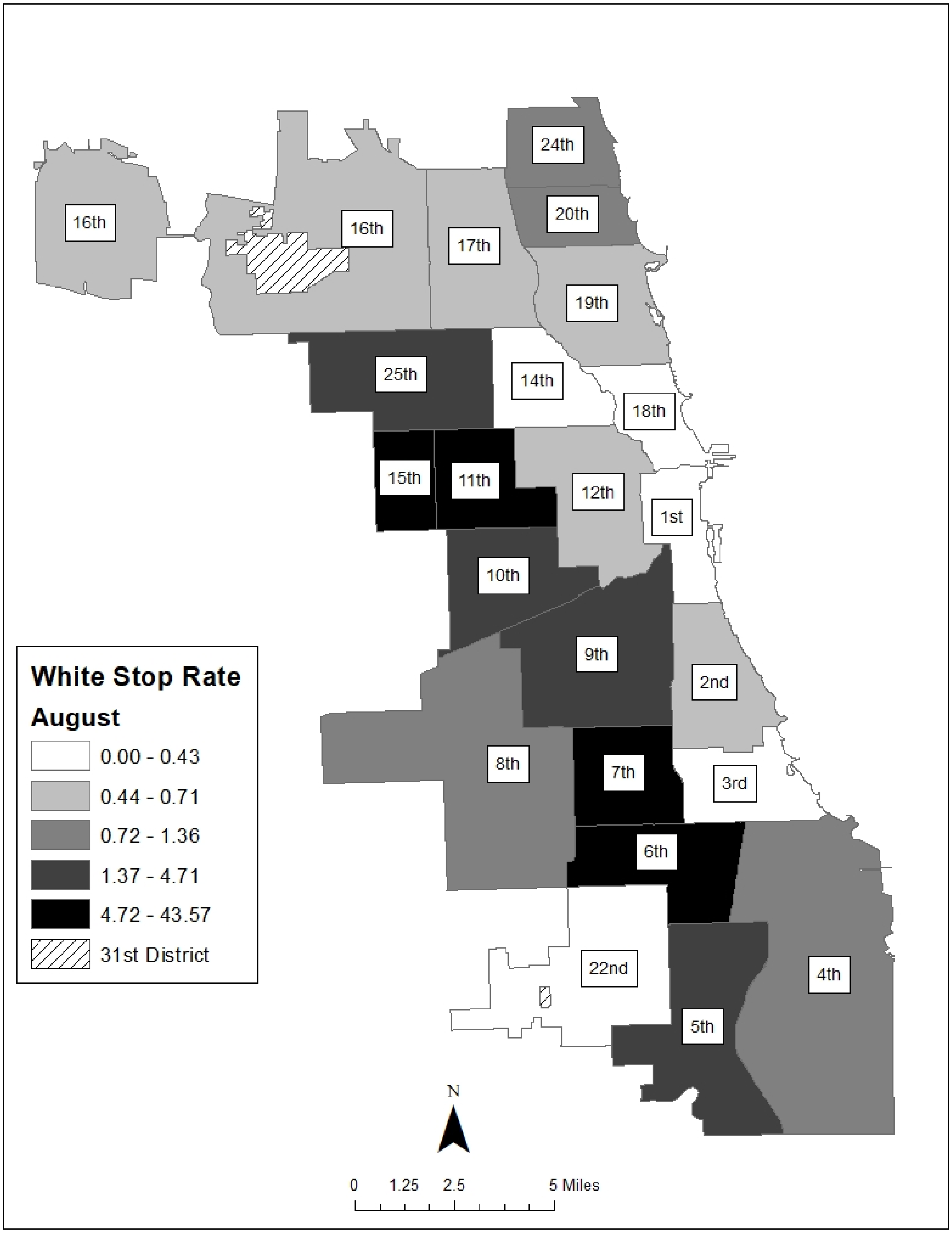
APPENDIX I: Black Stop Rate (per 1,000), December 2016



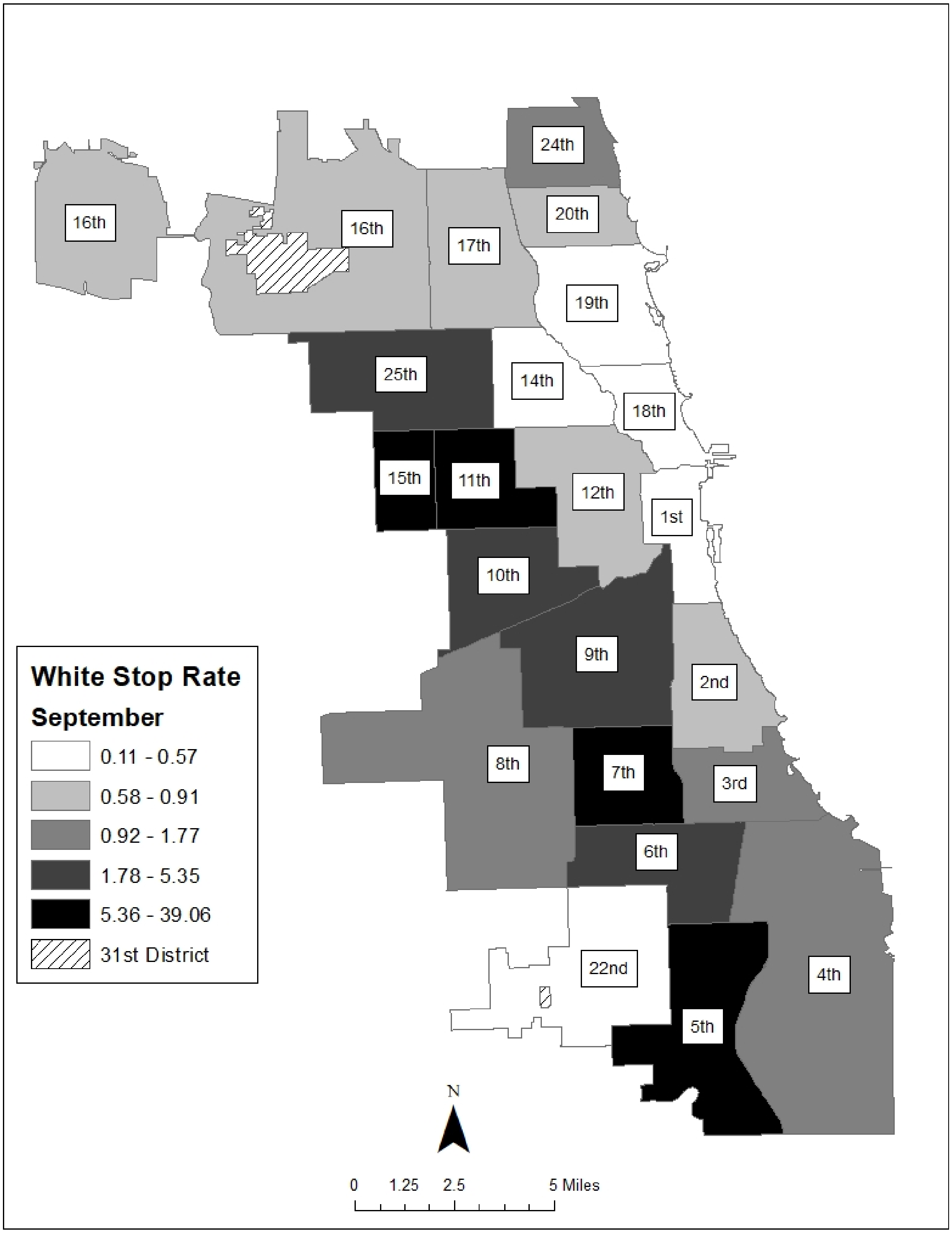
APPENDIX J: White Stop Rate (per 1,000), July 2016



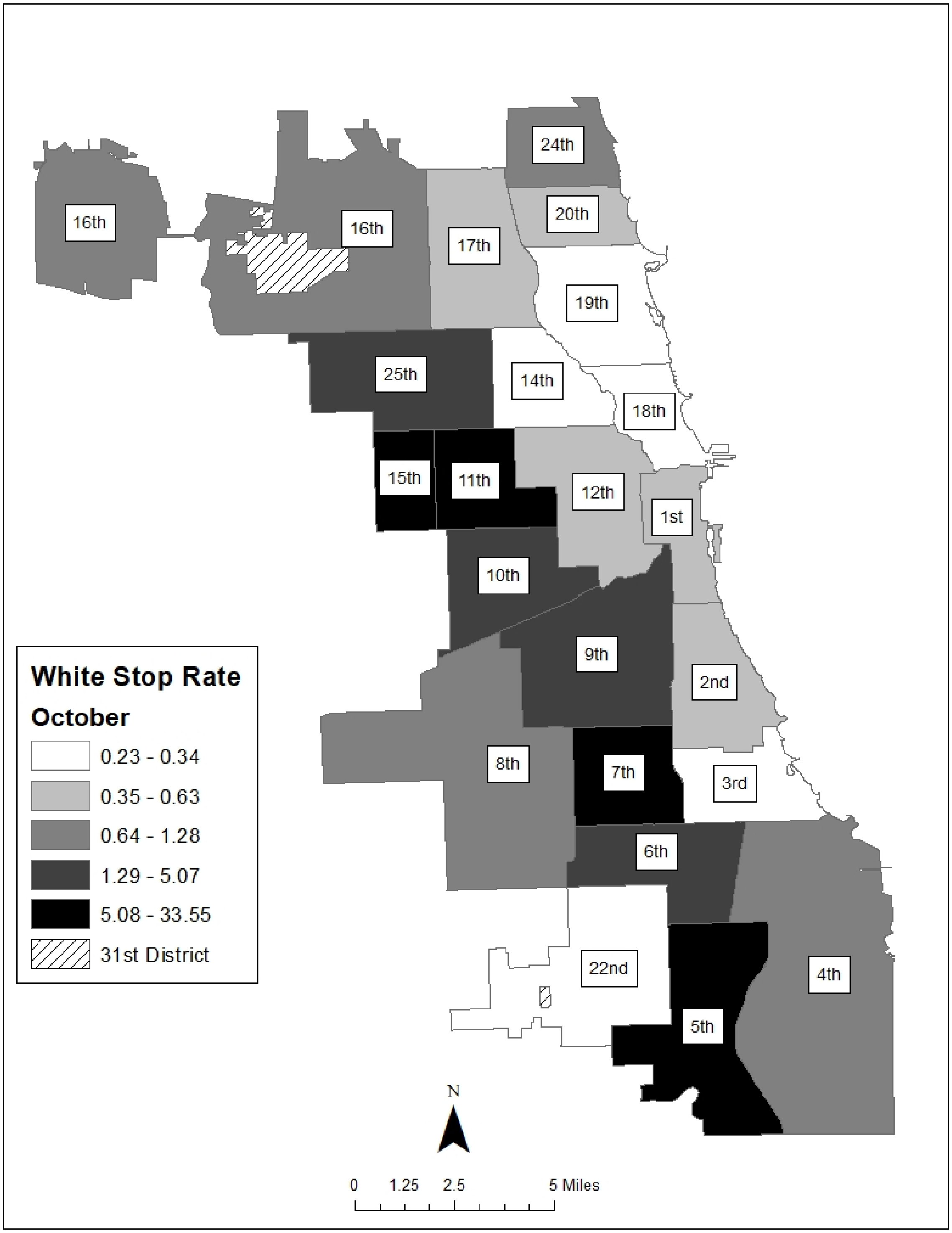
APPENDIX K: White Stop Rate (per 1,000), August 2016



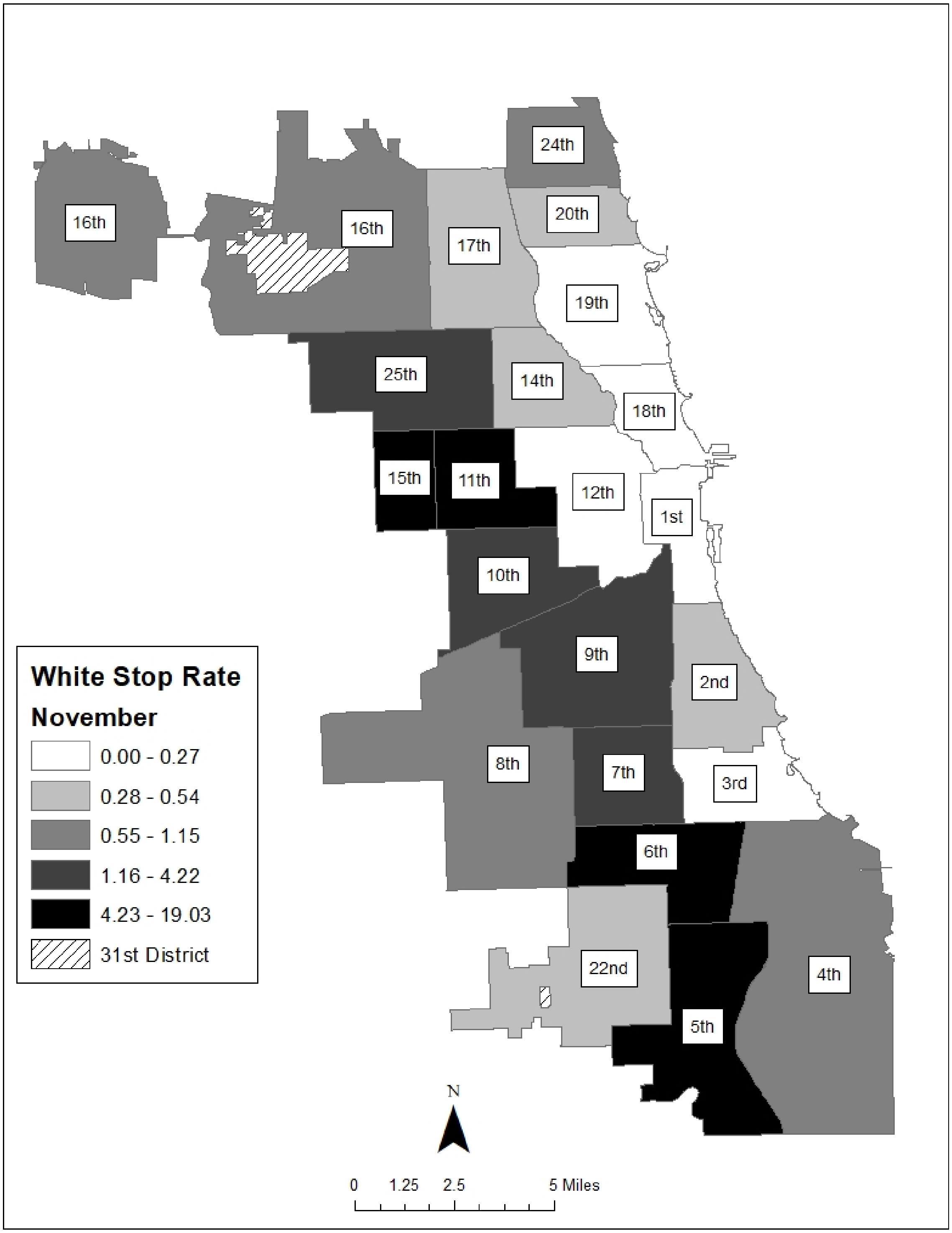
APPENDIX L: White Stop Rate (per 1,000), September 2016



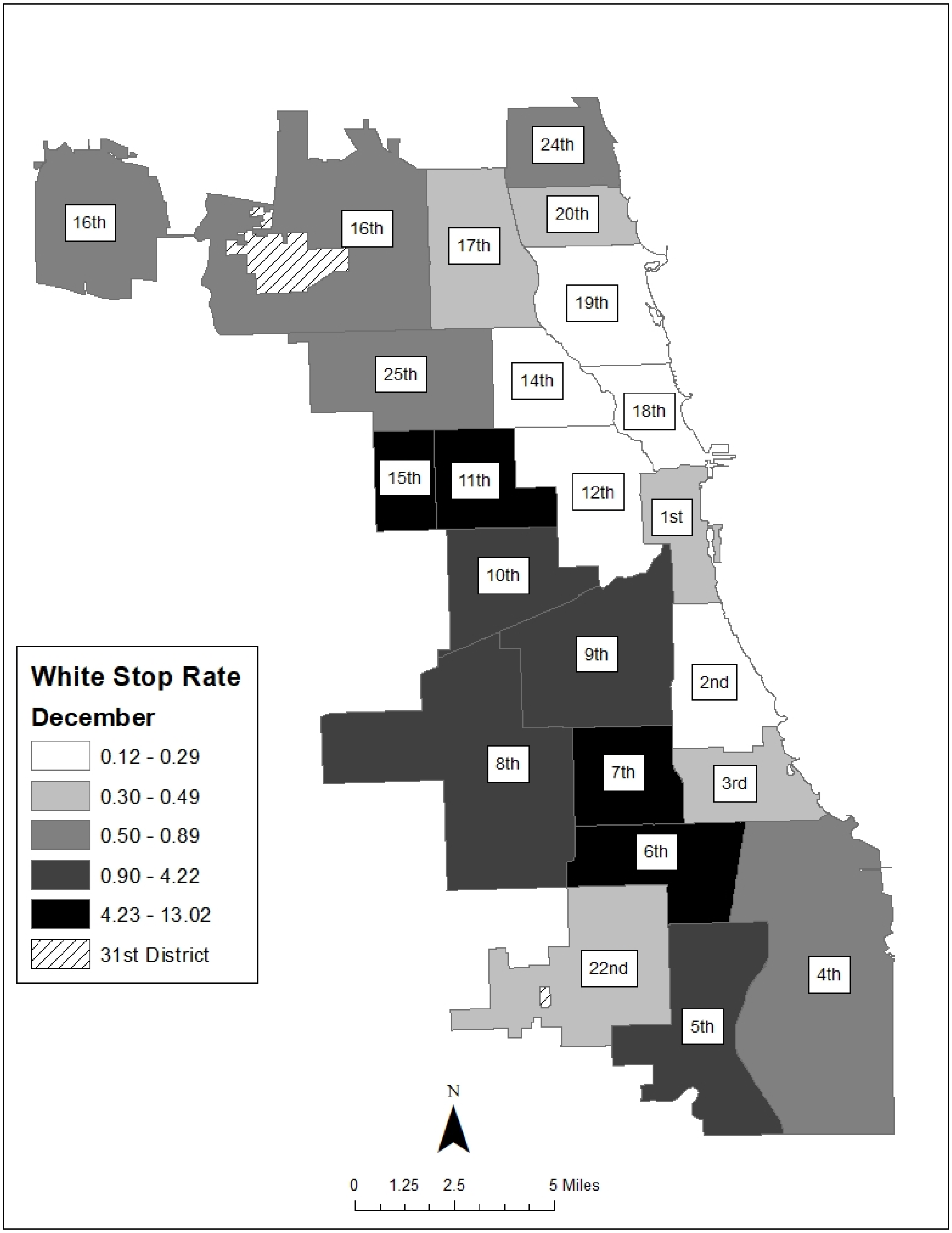
APPENDIX M: White Stop Rate (per 1,000), October 2016



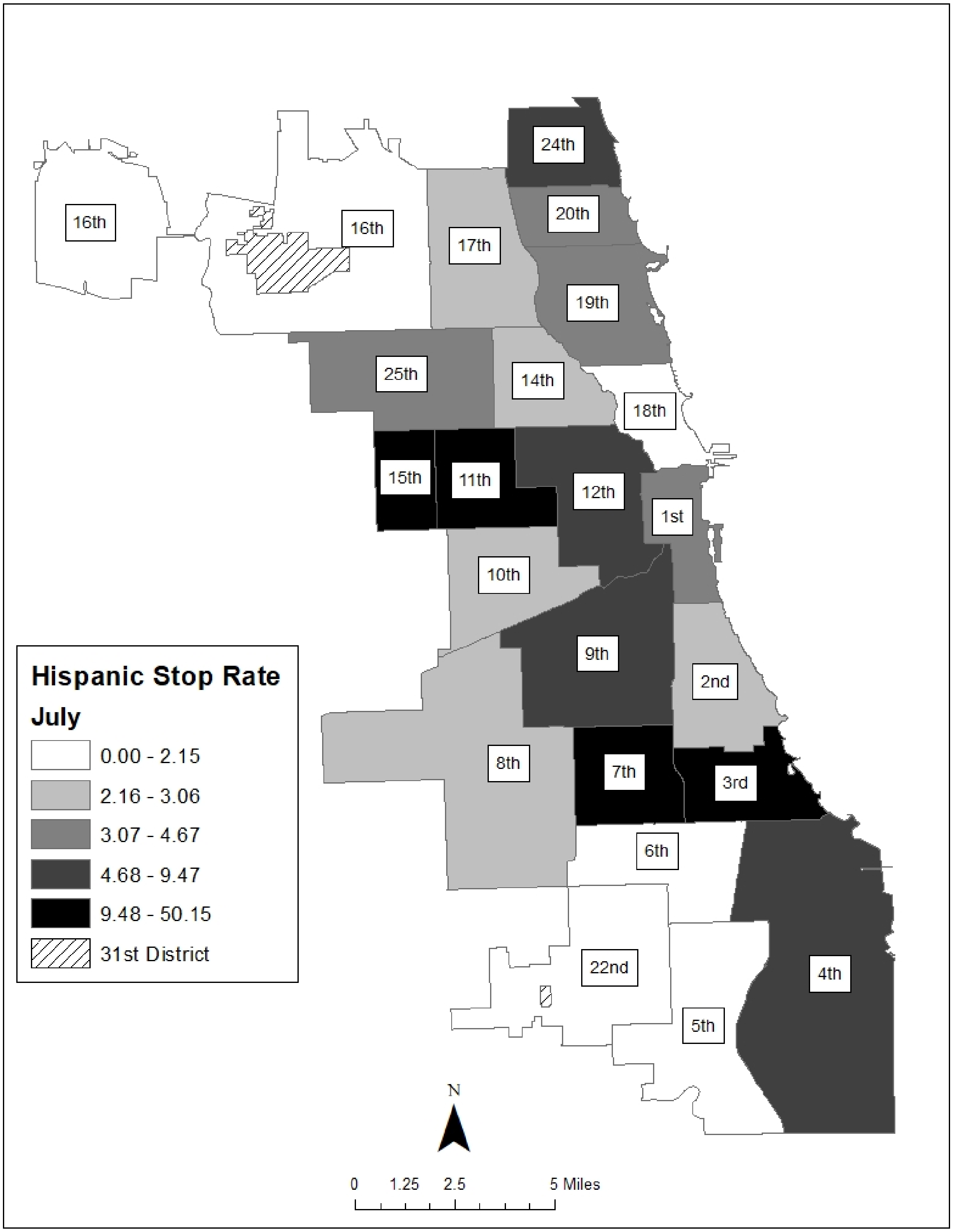
APPENDIX N: White Stop Rate (per 1,000), November 2016



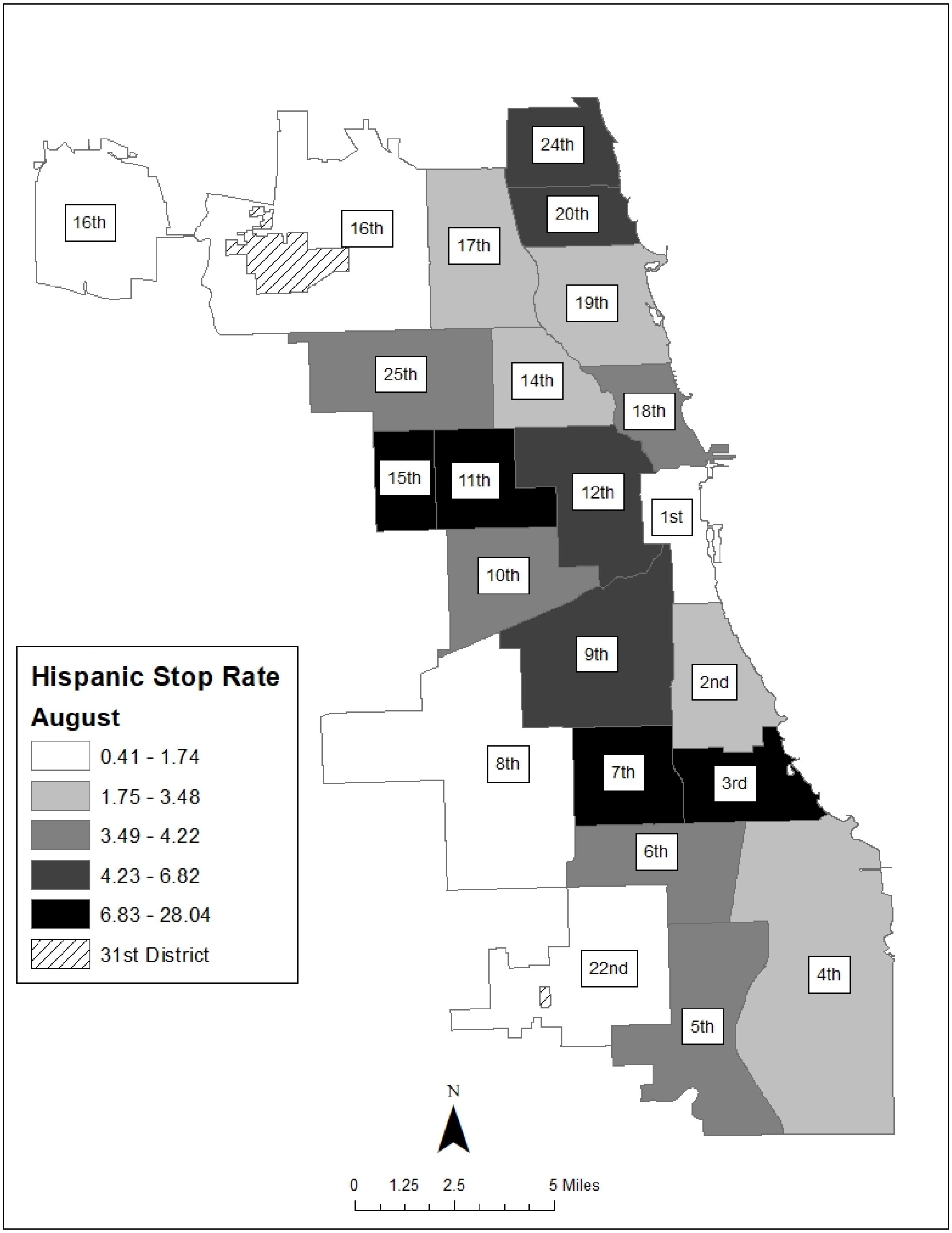
APPENDIX O: White Stop Rate (per 1,000), December 2016



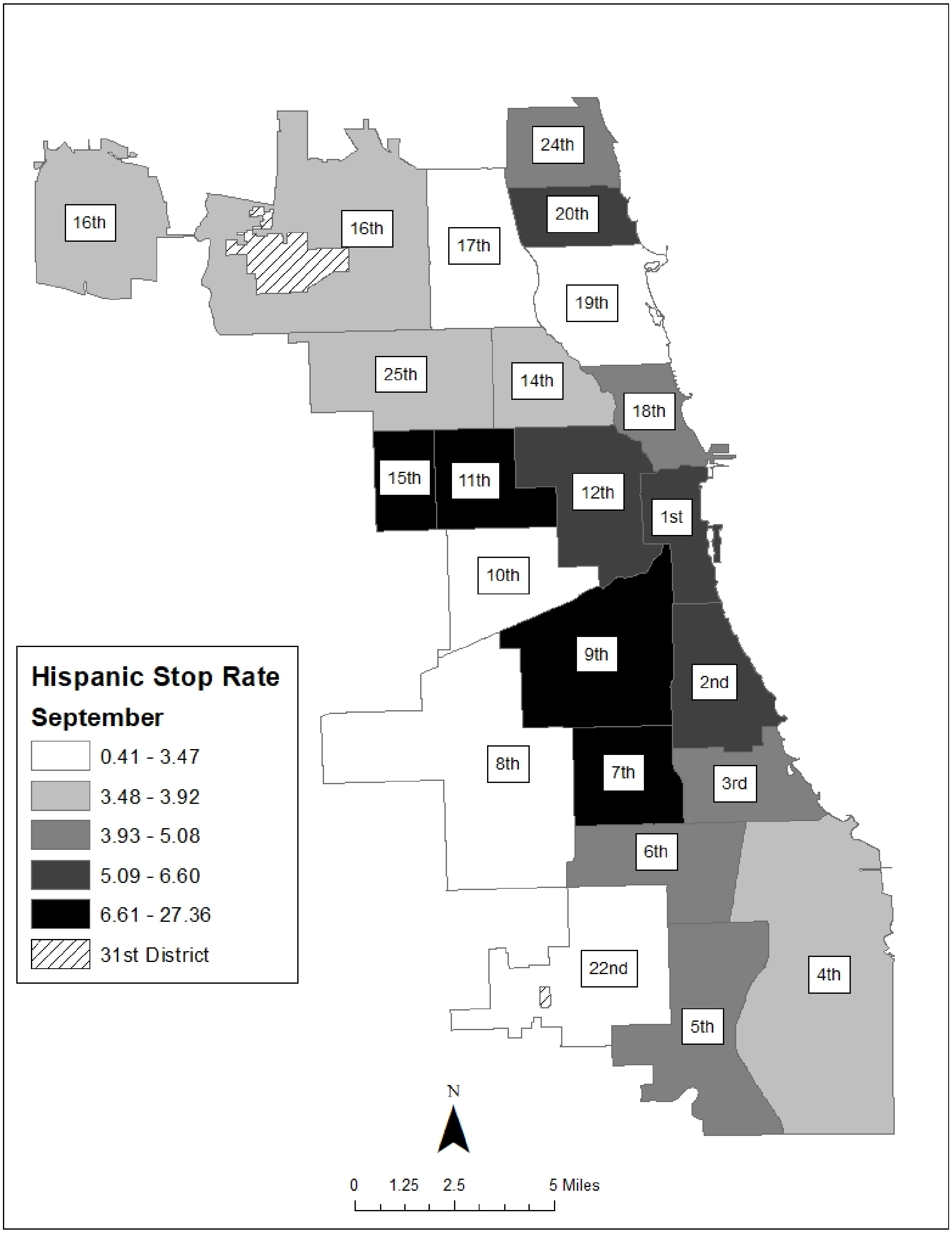
APPENDIX P: Hispanic Stop Rate (per 1,000), July 2016



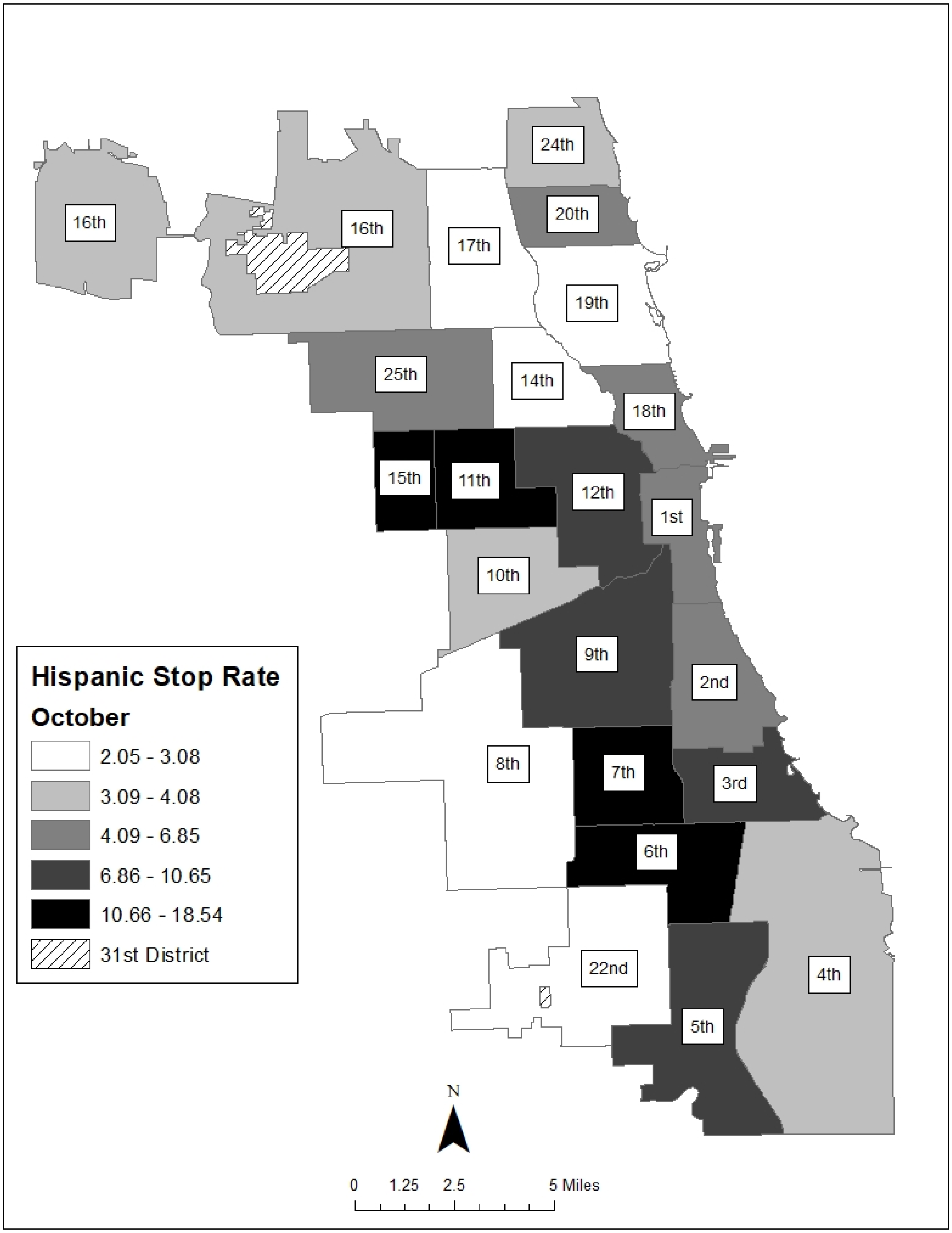
APPENDIX Q: Hispanic Stop Rate (per 1,000), August 2016



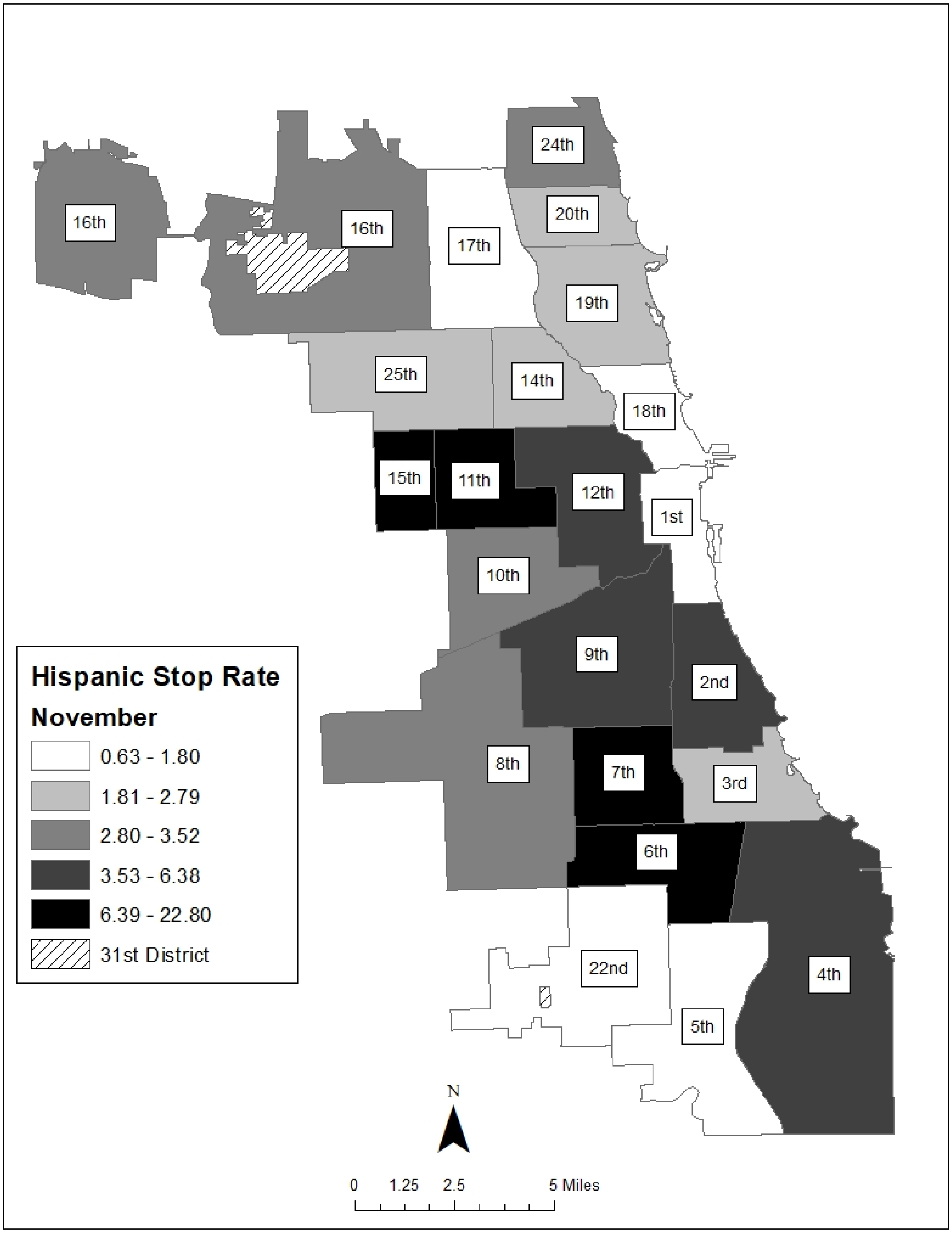
APPENDIX R: Hispanic Stop Rate (per 1,000), September 2016



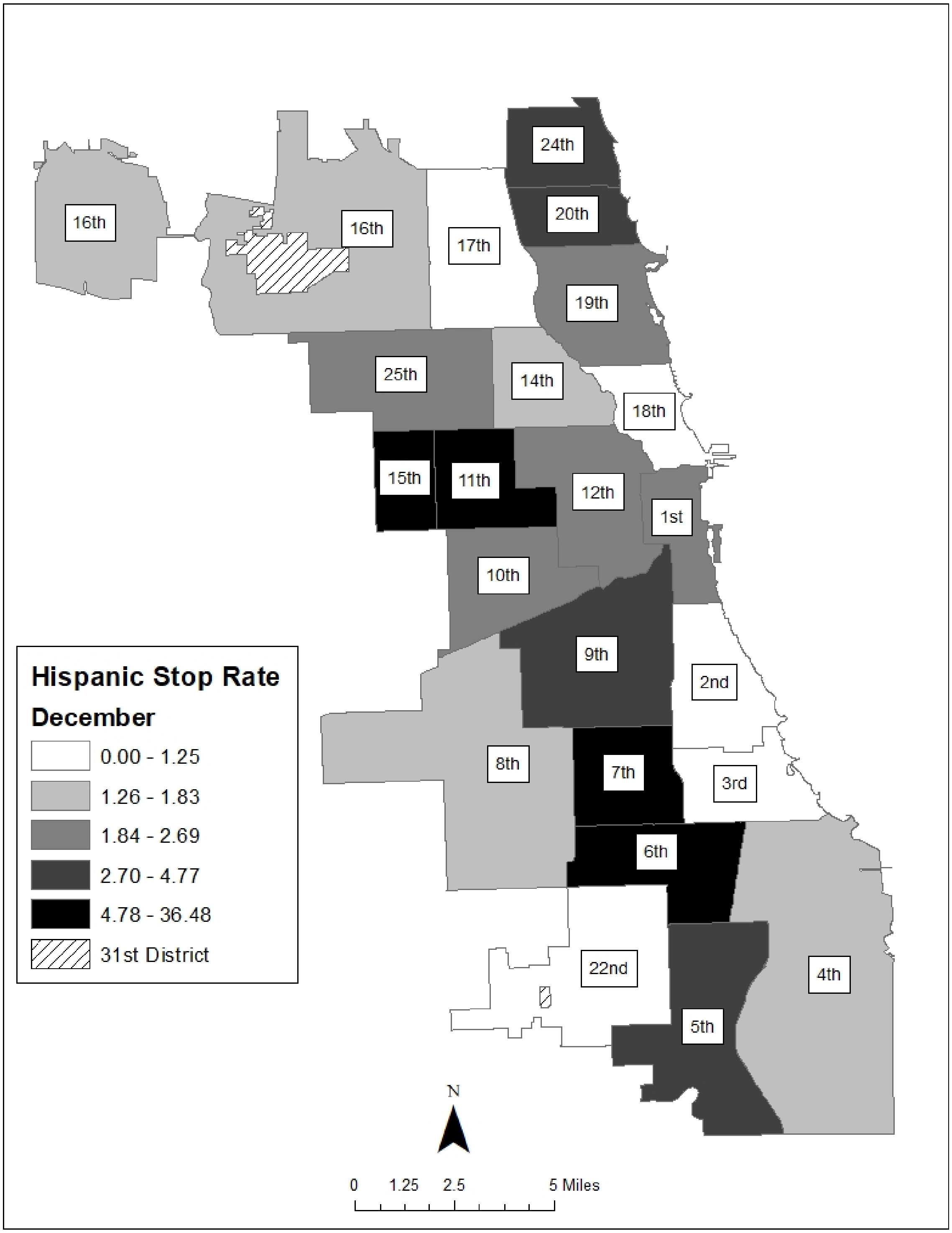
APPENDIX S: Hispanic Stop Rate (per 1,000), October 2016



APPENDIX T: Hispanic Stop Rate (per 1,000), November 2016



APPENDIX U: Hispanic Stop Rate (per 1,000), December 2016



APPENDIX V: Descriptive Statistics

Variable	n	Mean	Std. Dev.	Min	Max
Stop Count	396	127.88	182.12	0.00	1,352.00
Stop Count Lag	396	391.90	160.09	98.00	1,027.50
Black	396	0.33	0.47	0.00	1.00
Hispanic	396	0.33	0.47	0.00	1.00
Time	396	2.50	1.71	0.00	5.00
August	396	0.17	0.37	0.00	1.00
September	396	0.17	0.37	0.00	1.00
October	396	0.17	0.37	0.00	1.00
November	396	0.17	0.37	0.00	1.00
December	396	0.17	0.37	0.00	1.00
Percent Black	396	0.41	0.36	0.01	0.96
Socioeconomic Status	396	0.00	0.93	-1.52	1.63
Stability	396	0.00	0.81	-1.32	2.18
Violent Arrest Count[1]	396	4.86	4.71	1.00	21.00
Total Arrest Count[2]	396	105.80	143.55	1.00	869.00
Age-Weighted Population	396	4,782.79	2,143.31	2,088.58	9,656.59

Note: [1]Violent arrest count is transformed by +1, and spatial empirical Bayesian smoothed. [2]Total arrest count is transformed by +1. Sources: 2011-2015 American Community Survey; 2016 Chicago Police Department Investigatory Stop Reports, Arrest Data.

District	Month	Counts				Rates per 1,000 population			
		Total	Black	White	Hispanic	All	Black	White	Hispanic
1	July	119	77	30	11	1.68	5.25	0.85	3.96
1	August	124	106	14	4	1.75	7.22	0.40	1.44
1	September	120	82	20	16	1.69	5.59	0.57	5.76
1	October	111	79	12	19	1.57	5.38	0.34	6.85
1	November	61	47	9	5	0.86	3.20	0.26	1.80
1	December	87	65	14	7	1.23	4.43	0.40	2.52
2	July	564	532	16	5	5.88	8.12	0.91	2.90
2	August	612	584	11	6	6.38	8.92	0.63	3.48
2	September	555	532	11	10	5.79	8.12	0.63	5.80
2	October	519	491	10	10	5.41	7.50	0.57	5.80
2	November	477	452	9	11	4.97	6.90	0.51	6.38
2	December	351	341	5	1	3.66	5.21	0.29	0.58
3	July	335	319	6	5	4.26	4.47	1.89	10.65
3	August	291	284	0	4	3.70	3.98	0.00	8.52
3	September	328	319	5	2	4.17	4.47	1.57	4.26
3	October	270	259	1	5	3.43	3.63	0.31	10.65
3	November	202	201	0	1	2.57	2.82	0.00	2.13
3	December	164	161	1	0	2.09	2.26	0.31	0.00
4	July	582	433	16	128	4.89	5.93	1.58	4.69
4	August	423	335	8	79	3.55	4.58	0.79	2.90
4	September	521	392	18	106	4.38	5.36	1.77	3.89
4	October	590	484	12	87	4.96	6.62	1.18	3.19
4	November	518	398	9	109	4.35	5.45	0.89	4.00
4	December	400	340	9	50	3.36	4.65	0.89	1.83
5	July	472	464	4	1	6.64	6.97	3.14	0.63
5	August	457	442	6	6	6.43	6.64	4.71	3.81
5	September	553	530	11	8	7.78	7.96	8.64	5.08
5	October	415	387	12	14	5.84	5.81	9.42	8.88
5	November	316	306	6	1	4.45	4.59	4.71	0.63
5	December	247	234	3	7	3.47	3.51	2.36	4.44
6	July	241	239	2	0	2.73	2.81	2.41	0.00
6	August	303	292	8	1	3.43	3.43	9.63	4.22
6	September	378	366	3	1	4.28	4.30	3.61	4.22
6	October	362	353	4	3	4.10	4.15	4.82	12.66
6	November	418	403	8	3	4.73	4.74	9.63	12.66
6	December	315	302	6	2	3.57	3.55	7.22	8.44
7	July	654	613	13	22	10.46	10.45	21.10	50.15
7	August	600	572	11	6	9.60	9.75	17.85	13.68
7	September	666	646	4	12	10.65	11.01	6.49	27.36

7	October	655	633	9	6	10.48	10.79	14.61	13.68
7	November	479	462	2	10	7.66	7.87	3.25	22.80
7	December	317	294	5	16	5.07	5.01	8.12	36.48
8	July	540	295	74	169	2.15	5.89	1.54	2.60
8	August	471	292	65	113	1.87	5.83	1.36	1.74
8	September	525	293	55	172	2.09	5.85	1.15	2.65
8	October	513	255	57	200	2.04	5.09	1.19	3.08
8	November	476	216	55	204	1.89	4.31	1.15	3.14
8	December	306	138	51	115	1.22	2.75	1.06	1.77
9	July	659	246	60	351	3.96	14.79	2.52	6.40
9	August	582	221	47	307	3.50	13.29	1.97	5.59
9	September	660	226	62	366	3.97	13.59	2.60	6.67
9	October	912	304	79	520	5.48	18.28	3.31	9.47
9	November	522	175	46	294	3.14	10.52	1.93	5.36
9	December	444	141	33	262	2.67	8.48	1.38	4.77
10	July	393	240	15	134	3.62	6.98	4.22	2.86
10	August	534	343	14	173	4.91	9.98	3.94	3.69
10	September	778	631	19	126	7.16	18.36	5.35	2.69
10	October	618	402	18	191	5.69	11.70	5.07	4.08
10	November	738	560	15	160	6.79	16.30	4.22	3.42
10	December	728	607	15	100	6.70	17.66	4.22	2.14
11	July	1604	1352	152	82	22.40	22.70	76.12	29.23
11	August	1196	1057	87	48	16.70	17.75	43.57	17.11
11	September	962	838	78	43	13.43	14.07	39.06	15.33
11	October	1336	1212	67	52	18.66	20.35	33.55	18.54
11	November	839	766	38	29	11.72	12.86	19.03	10.34
11	December	771	700	26	40	10.77	11.75	13.02	14.26
12	July	420	145	56	215	3.15	6.06	1.00	9.47
12	August	300	114	30	155	2.25	4.76	0.53	6.82
12	September	371	179	37	150	2.79	7.48	0.66	6.60
12	October	398	193	29	172	2.99	8.06	0.52	7.57
12	November	307	175	15	117	2.30	7.31	0.27	5.15
12	December	177	97	16	61	1.33	4.05	0.29	2.69
14	July	186	60	17	108	1.53	7.01	0.30	3.06
14	August	188	59	24	104	1.54	6.89	0.43	2.95
14	September	215	66	25	123	1.77	7.71	0.45	3.48
14	October	174	60	19	93	1.43	7.01	0.34	2.63
14	November	133	45	16	71	1.09	5.26	0.29	2.01
14	December	103	38	10	54	0.85	4.44	0.18	1.53
15	July	673	641	10	21	11.45	11.95	8.16	24.53
15	August	773	723	22	24	13.15	13.48	17.96	28.04
15	September	547	512	12	19	9.30	9.54	9.80	22.20

15	October	507	477	11	11	8.62	8.89	8.98	12.85
15	November	549	517	10	18	9.34	9.64	8.16	21.03
15	December	535	504	15	15	9.10	9.40	12.24	17.52
16	July	244	64	111	61	1.19	24.33	0.82	2.15
16	August	201	48	97	46	0.98	18.25	0.71	1.62
16	September	289	52	124	102	1.41	19.77	0.91	3.59
16	October	374	88	174	99	1.83	33.45	1.28	3.48
16	November	282	62	127	81	1.38	23.57	0.94	2.85
16	December	222	67	104	42	1.08	25.47	0.77	1.48
17	July	245	53	55	129	1.64	12.14	0.96	3.03
17	August	181	36	40	97	1.21	8.25	0.70	2.28
17	September	158	24	35	96	1.06	5.50	0.61	2.26
17	October	148	27	28	88	0.99	6.19	0.49	2.07
17	November	116	14	31	67	0.78	3.21	0.54	1.58
17	December	78	19	20	39	0.52	4.35	0.35	0.92
18	July	183	147	25	9	1.49	14.81	0.28	1.87
18	August	152	107	26	17	1.24	10.78	0.29	3.54
18	September	172	130	20	21	1.40	13.10	0.22	4.37
18	October	213	163	26	20	1.74	16.43	0.29	4.16
18	November	128	110	14	4	1.04	11.09	0.15	0.83
18	December	130	111	11	6	1.06	11.19	0.12	1.25
19	July	311	158	70	74	1.49	12.51	0.45	4.67
19	August	257	137	69	48	1.23	10.85	0.44	3.03
19	September	279	154	65	55	1.34	12.20	0.42	3.47
19	October	224	120	53	47	1.08	9.50	0.34	2.97
19	November	157	79	38	35	0.75	6.26	0.24	2.21
19	December	153	70	37	42	0.74	5.54	0.24	2.65
20	July	209	93	54	46	2.34	8.75	1.10	4.53
20	August	161	57	40	52	1.80	5.36	0.82	5.12
20	September	187	83	35	63	2.10	7.81	0.71	6.21
20	October	155	56	31	59	1.74	5.27	0.63	5.81
20	November	115	61	19	28	1.29	5.74	0.39	2.76
20	December	118	54	24	30	1.32	5.08	0.49	2.96
22	July	188	158	27	2	1.82	2.56	0.77	0.82
22	August	185	170	14	1	1.79	2.75	0.40	0.41
22	September	175	165	4	1	1.70	2.67	0.11	0.41
22	October	197	181	8	5	1.91	2.93	0.23	2.05
22	November	137	121	10	2	1.33	1.96	0.29	0.82
22	December	121	105	11	3	1.17	1.70	0.31	1.23
24	July	327	173	47	98	2.34	7.24	0.76	4.72
24	August	320	133	76	102	2.29	5.57	1.23	4.91
24	September	340	182	59	84	2.44	7.62	0.95	4.05

24	October	325	152	79	83	2.33	6.36	1.28	4.00
24	November	292	149	61	73	2.09	6.24	0.98	3.52
24	December	210	105	32	66	1.50	4.40	0.52	3.18
25	July	451	197	48	202	2.27	6.38	1.74	3.34
25	August	542	248	61	225	2.73	8.03	2.21	3.72
25	September	482	182	60	237	2.43	5.90	2.18	3.92
25	October	506	179	63	262	2.55	5.80	2.29	4.33
25	November	385	177	38	169	1.94	5.73	1.38	2.79
25	December	388	212	23	152	1.96	6.87	0.84	2.51

Sources: 2011-2015 American Community Survey; 2016 Chicago Police Department Investigatory Stop Reports.

APPENDIX X: District-Level Stops Per 100 Previous Month's Arrests, July – December 2016

District	Month	Rates per 100 Previous Month's Violent Arrests				Rates per 100 Previous Month's Total Arrests			
		Total	Black	White	Hispanic	All	Black	White	Hispanic
1	July	991.67	819.15	1,363.64	348.39	39.80	37.75	57.69	28.21
1	August	1,771.43	1,354.48	636.36	133.33	39.49	50.00	25.00	9.52
1	September	1,500.00	836.73	1,250.00	1,120.00	40.96	36.94	42.55	80.00
1	October	1,110.00	782.60	666.67	633.33	35.02	34.35	25.53	51.35
1	November	610.00	586.35	750.00	185.13	19.68	20.70	20.93	15.15
1	December	870.00	928.57	700.00	291.67	32.34	31.86	42.42	22.58
2	July	4,700.00	4,290.32	888.89	280.00	155.80	154.20	266.67	45.45
2	August	6,120.00	4,918.36	600.00	283.42	231.82	242.32	110.00	54.55
2	September	11,100.00	5,097.99	785.71	641.38	233.19	238.57	122.22	166.67
2	October	2,883.33	3,366.12	833.33	615.38	190.81	197.98	90.91	90.91
2	November	6,814.29	4,911.84	900.00	472.81	166.20	166.79	112.50	157.14
2	December	11,700.00	4,481.31	272.73	41.67	146.25	151.56	71.43	11.11
3	July	3,045.45	2,184.93	500.00	312.50	72.98	69.80	600.00	166.67
3	August	1,531.58	1,535.59	0.00	222.22	83.86	85.54	0.00	50.00
3	September	3,644.44	2,654.52	500.00	142.86	106.84	104.25	250.00	100.00
3	October	1,687.50	1,488.51	100.00	357.14	87.10	84.09	33.33	250.00
3	November	1,442.86	1,356.16	0.00	55.56	77.10	78.52	0.00	50.00
3	December	1,640.00	1,916.67	83.33	0.00	62.60	61.92	33.33	0.00
4	July	4,157.14	2,886.67	1,280.00	8,533.33	155.20	141.04	123.08	232.73
4	August	2,226.32	1,763.16	640.00	4,514.29	123.32	117.96	200.00	149.06
4	September	3,473.33	2,570.49	1,800.00	8,480.00	185.41	160.00	163.64	378.57
4	October	2,681.82	2,889.55	1,200.00	4,971.43	209.96	195.16	133.33	348.00
4	November	2,877.78	2,211.11	900.00	6,228.57	153.25	147.41	69.23	194.64
4	December	4,000.00	2,775.51	720.00	5,000.00	129.87	132.81	81.82	125.00
5	July	3,146.67	3,145.76	320.00	66.67	112.92	113.45	80.00	14.29
5	August	2,688.24	2,618.92	600.00	342.86	109.59	109.95	120.00	46.15
5	September	3,072.22	3,452.16	880.00	640.00	138.25	136.60	122.22	133.33
5	October	3,192.31	2,496.77	1,200.00	800.00	101.22	97.24	150.00	200.00
5	November	1,373.91	1,700.00	600.00	57.14	92.94	92.17	100.00	25.00
5	December	1,452.94	1,624.28	240.00	700.00	66.76	64.11	100.00	175.00
6	July	1,268.42	1,689.90	155.56	0.00	57.38	58.01	33.33	0.00
6	August	1,377.27	1,566.80	700.00	50.00	73.72	72.82	114.29	20.00
6	September	2,223.53	2,510.78	210.00	65.71	93.80	92.42	60.00	33.33
6	October	2,262.50	2,438.96	311.11	205.57	83.60	82.09	133.33	300.00
6	November	2,458.82	2,390.68	800.00	175.00	85.66	83.61	400.00	50.00
6	December	3,500.00	2,830.17	420.00	144.40	69.38	67.56	66.67	200.00

7	July	4,087.50	4,431.33	709.09	872.44	115.34	110.65	118.18	550.00
7	August	2,608.70	2,910.36	641.67	253.70	116.28	112.38	366.67	150.00
7	September	4,440.00	4,941.80	240.00	477.67	156.34	157.95	80.00	85.71
7	October	5,038.46	4,559.99	600.00	362.64	116.13	115.51	112.50	60.00
7	November	2,817.65	3,052.64	200.00	632.65	96.77	96.05	40.00	90.91
7	December	4,528.57	3,312.08	269.23	677.97	81.28	77.37	166.67	160.00
8	July	2,700.00	2,347.39	4,036.36	2,168.19	139.90	151.28	168.18	117.36
8	August	3,364.29	2,262.78	4,333.33	2,489.83	119.54	135.81	191.18	79.02
8	September	4,038.46	3,298.39	3,000.00	2,764.29	146.24	173.37	87.30	135.43
8	October	2,850.00	2,172.73	2,850.00	2,167.68	132.90	146.55	126.67	122.70
8	November	3,400.00	1,631.48	5,500.00	6,251.61	138.37	121.35	130.95	171.43
8	December	1,092.86	949.32	2,380.00	1,726.13	97.76	86.79	121.43	102.68
9	July	3,138.10	2,236.36	3,000.00	4,601.80	154.33	156.69	150.00	158.82
9	August	2,910.00	1,826.91	2,277.32	6,258.40	146.97	136.42	127.03	156.63
9	September	2,869.57	2,021.75	3,616.67	5,897.50	167.94	138.65	119.23	212.79
9	October	5,364.71	2,891.98	3,950.00	6,811.98	224.08	187.65	175.56	266.67
9	November	3,728.57	2,359.50	4,025.00	3,986.44	146.63	126.81	143.75	158.06
9	December	2,336.84	1,186.57	1,650.00	6,550.00	160.87	114.63	165.00	200.00
10	July	2,620.00	2,578.93	681.82	1,617.24	88.71	95.24	115.38	74.86
10	August	2,225.00	2,170.72	636.36	3,052.94	129.61	149.13	56.00	108.13
10	September	7,072.73	6,170.84	950.00	3,015.18	192.10	240.84	111.76	99.21
10	October	2,472.00	3,033.96	750.00	3,225.01	137.33	148.89	75.00	125.66
10	November	4,920.00	6,036.80	1,250.00	2,733.67	173.65	212.93	100.00	109.59
10	December	3,466.67	3,927.65	625.00	1,943.40	161.42	181.74	166.67	94.34
11	July	6,973.91	7,350.37	11,400.00	1,979.89	165.36	159.25	276.36	141.38
11	August	4,983.33	5,033.33	5,220.00	1,495.08	127.78	129.85	127.94	87.27
11	September	5,063.16	5,224.55	5,850.00	1,907.88	108.09	108.41	144.44	76.79
11	October	12,145.45	11,432.09	3,092.31	1,747.54	135.91	139.47	134.00	85.25
11	November	6,453.85	8,598.63	2,280.00	748.39	105.01	106.83	102.70	64.44
11	December	3,504.55	3,578.48	2,228.57	1,517.24	99.10	99.57	66.67	129.03
12	July	4,666.67	1,724.79	3,015.38	7,053.87	161.54	100.69	121.74	325.76
12	August	5,000.00	2,217.34	1,312.50	4,408.51	128.76	85.07	73.17	276.79
12	September	3,710.00	1,736.36	2,590.00	7,840.91	177.51	142.06	132.14	267.86
12	October	5,685.71	2,299.64	1,353.33	7,250.59	203.06	160.83	93.55	390.91
12	November	7,675.00	2,512.57	1,312.50	3,059.25	188.34	190.22	55.56	272.09
12	December	3,540.00	1,504.39	861.54	2,033.33	93.16	79.51	61.54	148.78
14	July	2,066.67	1,474.67	991.67	2,209.09	89.86	111.11	68.00	85.71
14	August	4,700.00	2,837.44	1,120.00	4,282.35	109.94	143.90	80.00	108.33
14	September	2,687.50	1,387.86	1,166.67	3,323.56	128.74	137.50	113.64	136.67
14	October	2,485.71	990.34	831.25	3,202.36	127.94	153.85	86.36	132.86
14	November	1,477.78	612.64	1,018.18	1,736.61	95.00	102.27	59.26	102.90
14	December	2,060.00	816.97	875.00	2,362.50	82.40	118.75	29.41	93.10

15	July	3,365.00	3,731.39	1,000.00	525.00	135.69	138.15	62.50	110.53
15	August	3,865.00	4,009.28	1,650.00	900.00	152.47	152.21	137.50	141.18
15	September	6,837.50	4,442.80	900.00	1,085.71	124.32	127.68	66.67	86.36
15	October	3,621.43	3,927.63	660.00	481.25	132.38	131.77	183.33	64.71
15	November	13,725.00	5,965.38	500.00	837.50	143.34	142.82	142.86	120.00
15	December	7,642.86	5,922.23	1,125.00	800.00	117.84	117.21	150.00	100.00
16	July	4,880.00	3,878.79	4,757.14	1,307.14	132.61	130.61	135.37	117.31
16	August	6,700.00	1,557.89	4,850.00	1,725.00	108.65	133.33	110.23	82.14
16	September	9,633.33	2,510.34	4,133.33	3,290.32	173.05	144.44	163.16	217.02
16	October	37,400.00	2,464.00	8,700.00	2,985.71	221.30	400.00	202.33	198.00
16	November	14,100.00	1,458.82	6,350.00	4,284.78	176.25	172.22	169.33	176.09
16	December	3,171.43	2,512.50	6,240.00	1,145.45	153.10	223.33	167.74	93.33
17	July	4,900.00	2,177.76	2,750.00	2,935.05	156.05	230.43	117.02	159.26
17	August	3,016.67	819.75	2,000.00	4,243.75	150.83	124.14	121.21	183.02
17	September	2,257.14	644.93	1,441.18	3,118.78	127.42	114.29	109.38	147.69
17	October	1,850.00	572.73	1,400.00	2,933.33	104.96	79.41	100.00	112.82
17	November	3,866.67	227.91	1,972.73	2,206.10	93.55	66.67	103.33	101.52
17	December	2,600.00	449.46	1,400.00	1,436.84	69.03	105.56	54.05	73.58
18	July	3,050.00	2,227.27	1,250.00	278.39	80.97	103.52	45.45	29.03
18	August	2,171.43	2,219.50	1,300.00	772.73	88.37	96.40	68.42	89.47
18	September	2,150.00	1,756.76	1,111.11	1,145.45	74.78	76.47	54.05	100.00
18	October	1,936.36	2,333.95	1,083.33	909.09	118.33	128.35	72.22	117.65
18	November	1,163.64	1,287.56	1,166.67	125.00	61.24	76.39	30.43	21.05
18	December	2,166.67	1,790.32	916.67	250.00	66.33	84.73	39.29	18.18
19	July	3,110.00	3,875.47	4,375.00	2,379.12	94.24	81.87	93.33	137.04
19	August	4,283.33	3,425.00	3,136.36	2,181.82	120.09	131.73	116.95	94.12
19	September	3,100.00	3,061.56	2,954.55	2,500.00	119.74	136.28	84.42	127.91
19	October	2,800.00	2,000.00	2,409.09	1,958.33	101.82	108.11	96.36	97.92
19	November	1,308.33	1,091.29	3,166.67	1,029.41	67.38	84.95	45.78	70.00
19	December	1,700.00	1,296.30	3,083.33	2,333.33	72.17	76.09	54.41	82.35
20	July	6,966.67	2,363.97	2,700.00	1,415.38	258.02	281.82	284.21	164.29
20	August	3,220.00	1,200.00	2,000.00	3,466.67	185.06	154.05	181.82	260.00
20	September	9,350.00	1,952.94	1,272.73	2,800.00	287.69	296.43	166.67	393.75
20	October	2,214.29	933.33	1,771.43	2,360.00	231.34	164.71	281.82	280.95
20	November	1,277.78	1,016.67	1,520.00	861.54	143.75	160.53	95.00	133.33
20	December	2,360.00	1,028.57	1,920.00	1,714.29	138.82	122.73	141.18	125.00
22	July	1,880.00	1,089.66	1,800.00	133.33	85.45	76.70	225.00	66.67
22	August	3,083.33	1,639.88	1,400.00	48.86	90.24	91.40	70.00	50.00
22	September	2,187.50	1,736.68	228.57	65.63	95.11	98.21	28.57	20.00
22	October	1,790.91	1,478.59	533.33	315.85	107.07	106.47	72.73	125.00
22	November	978.57	701.45	1,000.00	133.33	58.30	55.25	62.50	66.67
22	December	3,025.00	1,277.38	733.33	200.00	74.23	67.31	137.50	300.00

24	July	3,270.00	4,441.89	2,820.00	2,672.73	194.64	198.85	156.67	213.04
24	August	6,400.00	3,069.23	3,800.00	6,120.00	253.97	179.73	330.43	463.64
24	September	3,777.78	4,963.64	2,528.57	3,600.00	263.57	284.38	163.89	336.00
24	October	4,062.50	2,280.00	7,900.00	3,112.50	273.11	245.16	376.19	259.38
24	November	4,866.67	2,980.00	4,575.00	2,737.50	180.25	175.29	203.33	162.22
24	December	3,500.00	2,625.00	2,400.00	3,960.00	152.17	143.84	114.29	227.59
25	July	3,006.67	1,828.63	2,880.00	4,328.57	90.93	105.91	123.08	76.52
25	August	5,420.00	3,026.41	3,050.00	9,000.00	109.27	134.05	122.00	90.00
25	September	2,835.29	1,748.09	3,000.00	5,152.17	104.33	96.81	113.21	109.22
25	October	3,892.31	3,225.61	3,780.00	7,939.39	116.06	114.01	118.87	122.43
25	November	1,833.33	2,371.20	2,533.33	2,216.08	94.13	110.63	102.70	82.84
25	December	5,542.86	5,929.50	1,533.33	5,700.00	99.74	135.90	65.71	82.16

Sources: 2016 Chicago Police Department Investigatory Stop Reports and Arrest Data.

	Violent Arrests					Total Arrests					Age-Weighted Population				
	b	SE	IRR	LCI	UCI	b	SE	IRR	LCI	UCI	b	SE	IRR	LCI	UCI
Intercept	3.01	0.12	20.31	2.78	3.24	0.16	0.09	1.17	-0.01	0.33	-3.66	0.17	0.03	-3.99	-3.33
Ln(Exposure)	1.00					1.00					1.00				
Ln(alpha)	-0.65	0.07		-0.80	-0.50	-2.13	0.10		-2.32	-1.94	0.34	0.06		0.21	0.46
Level 2 Variance	0.26	0.09		0.14	0.51	0.15	0.05		0.08	0.28	0.55	0.19		0.28	1.08
Likelihood Ratio X^2	97.31	***				177.96	***				98.60	***			
AIC	4,108.76					3,626.68					4,551.55				
BIC	4,120.71					3,638.63					4,563.50				

Notes: N=396 district-months. ***p<.001. IRR - Incidence rate ratio. LCI - Lower confidence interval. UCI - Upper confidence interval. Sources: Chicago Police Department Investigatory Stop Reports and arrest data; 2011-2015 American Community Survey.

APPENDIX Z: RP1 Model C, Violent Arrests

	Model C			
	b	SE	IRR	
Intercept	0.56	0.10	1.75	
Black	0.81	0.10	2.26	***
Hispanic	0.25	0.09	1.29	**
Time	0.09	0.02	1.10	***
Percent Black	-1.73	0.32	0.18	***
SES	-0.48	0.15	0.62	***
Stability	-0.06	0.27	0.94	
Ln(Violent Arrests)	1.00			
Ln(alpha)	-0.68	0.07		***
Level 2 Variance	0.09	0.04		
Likelihood Ratio X^2	35.31			***
AIC	4,027.41			
BIC	4,063.04			

Notes: N=387 district-months. * p<.05, ** p<.01, *** p<.001. IRR – Incidence rate ratio. Exposure measure is ethnoracial-specific violent arrest count lagged by 1 month. Sources: 2010-2014 American Community Survey; 2016 Chicago Police Department Investigatory Stop Reports, and 2015-2016 arrest data.

APPENDIX AA: City-Level Violent Arrest Counts and Rates

Month	Counts				Rates per 10,000 population			
	All	Black	White	Hispanic	All	Black	White	Hispanic
June	280	203	15	59	1.03	2.42	0.17	1.32
July	277	223	15	36	1.02	2.66	0.17	0.80
August	242	186	16	35	0.89	2.22	0.18	0.78
September	274	209	16	43	1.01	2.49	0.18	0.96
October	257	202	5	48	0.95	2.41	0.06	1.07
November	220	168	12	33	0.81	2.00	0.14	0.74

District	Month	Counts				Rates per 10,000 population			
		All	Black	White	Hispanic	All	Black	White	Hispanic
1	June	12	9	1	2	1.69	6.13	0.28	7.21
1	July	7	7	0	0	0.99	4.77	0.00	0.00
1	August	8	8	0	0	1.13	5.45	0.00	0.00
1	September	10	8	0	2	1.41	5.45	0.00	7.21
1	October	10	9	0	1	1.41	6.13	0.00	3.60
1	November	10	6	1	3	1.41	4.09	0.28	10.81
2	June	12	12	0	0	1.25	1.83	0.00	0.00
2	July	10	9	0	1	1.04	1.37	0.00	5.80
2	August	5	5	0	0	0.52	0.76	0.00	0.00
2	September	18	18	0	0	1.88	2.75	0.00	0.00
2	October	7	6	0	1	0.73	0.92	0.00	5.80
2	November	3	3	0	0	0.31	0.46	0.00	0.00
3	June	11	11	0	0	1.40	1.54	0.00	0.00
3	July	19	18	1	0	2.42	2.52	3.15	0.00
3	August	9	9	0	0	1.14	1.26	0.00	0.00
3	September	16	16	0	0	2.03	2.24	0.00	0.00
3	October	14	14	0	0	1.78	1.96	0.00	0.00
3	November	10	10	0	0	1.27	1.40	0.00	0.00
4	June	14	12	0	2	1.18	1.64	0.00	0.73
4	July	19	17	0	2	1.60	2.33	0.00	0.73
4	August	15	14	0	1	1.26	1.92	0.00	0.37
4	September	22	19	0	2	1.85	2.60	0.00	0.73
4	October	18	15	0	3	1.51	2.05	0.00	1.10
4	November	10	9	1	0	0.84	1.23	0.98	0.00
5	June	15	15	0	0	2.11	2.25	0.00	0.00
5	July	17	17	0	0	2.39	2.55	0.00	0.00
5	August	18	18	0	0	2.53	2.70	0.00	0.00
5	September	13	12	0	1	1.83	1.80	0.00	6.34
5	October	23	22	0	0	3.24	3.30	0.00	0.00
5	November	17	17	0	0	2.39	2.55	0.00	0.00
6	June	19	18	1	0	2.15	2.12	12.04	0.00
6	July	22	20	0	1	2.49	2.35	0.00	42.19
6	August	17	16	0	0	1.93	1.88	0.00	0.00
6	September	16	16	0	0	1.81	1.88	0.00	0.00
6	October	17	17	0	0	1.93	2.00	0.00	0.00
6	November	9	9	0	0	1.02	1.06	0.00	0.00
7	June	16	15	0	1	2.56	2.56	0.00	22.80
7	July	23	23	0	0	3.68	3.92	0.00	0.00
7	August	15	14	0	1	2.40	2.39	0.00	22.80

7	September	13	13	0	0	2.08	2.22	0.00	0.00
7	October	17	17	0	0	2.72	2.90	0.00	0.00
7	November	7	6	0	1	1.12	1.02	0.00	22.80
8	June	20	11	1	8	0.79	2.20	0.21	1.23
8	July	14	10	0	4	0.56	2.00	0.00	0.62
8	August	13	4	2	7	0.52	0.80	0.42	1.08
8	September	18	6	2	10	0.72	1.20	0.42	1.54
8	October	14	12	0	2	0.56	2.40	0.00	0.31
8	November	28	17	2	8	1.11	3.39	0.42	1.23
9	June	21	10	3	8	1.26	6.01	1.26	1.46
9	July	20	11	4	5	1.20	6.61	1.68	0.91
9	August	23	14	2	7	1.38	8.42	0.84	1.28
9	September	17	8	1	8	1.02	4.81	0.42	1.46
9	October	14	5	0	9	0.84	3.01	0.00	1.64
9	November	19	12	4	3	1.14	7.22	1.68	0.55
10	June	15	6	0	9	1.38	1.75	0.00	1.92
10	July	24	16	0	8	2.21	4.66	0.00	1.71
10	August	11	8	0	3	1.01	2.33	0.00	0.64
10	September	25	17	3	5	2.30	4.95	8.45	1.07
10	October	15	9	0	6	1.38	2.62	0.00	1.28
10	November	21	15	0	5	1.93	4.37	0.00	1.07
11	June	23	20	0	2	3.21	3.36	0.00	7.13
11	July	24	22	1	1	3.35	3.69	5.01	3.57
11	August	19	19	0	0	2.65	3.19	0.00	0.00
11	September	11	10	0	1	1.54	1.68	0.00	3.57
11	October	13	11	0	2	1.82	1.85	0.00	7.13
11	November	22	21	1	0	3.07	3.53	5.01	0.00
12	June	9	7	2	0	0.68	2.92	0.36	0.00
12	July	6	3	1	2	0.45	1.25	0.18	0.88
12	August	10	9	1	0	0.75	3.76	0.18	0.00
12	September	7	6	1	0	0.53	2.51	0.18	0.00
12	October	4	1	1	2	0.30	0.42	0.18	0.88
12	November	5	4	0	1	0.38	1.67	0.00	0.44
14	June	9	2	0	7	0.74	2.34	0.00	1.98
14	July	4	0	2	2	0.33	0.00	0.36	0.57
14	August	8	2	0	4	0.66	2.34	0.00	1.13
14	September	7	2	1	3	0.58	2.34	0.18	0.85
14	October	9	6	0	3	0.74	7.01	0.00	0.85
14	November	5	3	0	2	0.41	3.50	0.00	0.57
15	June	20	18	0	2	3.40	3.36	0.00	23.37
15	July	20	18	0	1	3.40	3.36	0.00	11.68
15	August	8	8	0	0	1.36	1.49	0.00	0.00
15	September	14	14	0	0	2.38	2.61	0.00	0.00

15	October	4	4	0	0	0.68	0.75	0.00	0.00
15	November	7	7	0	0	1.19	1.30	0.00	0.00
16	June	5	0	3	2	0.24	0.00	0.22	0.70
16	July	3	0	1	2	0.15	0.00	0.07	0.70
16	August	3	0	2	1	0.15	0.00	0.15	0.35
16	September	1	0	1	0	0.05	0.00	0.07	0.00
16	October	2	2	0	0	0.10	7.60	0.00	0.00
16	November	7	1	2	3	0.34	3.80	0.15	1.06
17	June	5	0	1	4	0.33	0.00	0.17	0.94
17	July	6	4	2	0	0.40	9.17	0.35	0.00
17	August	7	2	3	1	0.47	4.58	0.52	0.24
17	September	8	5	0	3	0.54	11.46	0.00	0.71
17	October	3	2	0	1	0.20	4.58	0.00	0.24
17	November	3	2	0	1	0.20	4.58	0.00	0.24
18	June	6	4	0	2	0.49	4.03	0.00	4.16
18	July	7	4	1	2	0.57	4.03	0.11	4.16
18	August	8	8	0	0	0.65	8.06	0.00	0.00
18	September	11	9	2	0	0.90	9.07	0.22	0.00
18	October	11	10	0	1	0.90	10.08	0.00	2.08
18	November	6	5	0	0	0.49	5.04	0.00	0.00
19	June	10	6	2	1	0.48	4.75	0.13	0.63
19	July	6	5	1	0	0.29	3.96	0.06	0.00
19	August	9	5	3	1	0.43	3.96	0.19	0.63
19	September	8	3	3	1	0.38	2.38	0.19	0.63
19	October	12	8	0	4	0.58	6.34	0.00	2.53
19	November	9	8	0	1	0.43	6.34	0.00	0.63
20	June	3	2	0	0	0.34	1.88	0.00	0.00
20	July	5	2	0	2	0.56	1.88	0.00	1.97
20	August	2	1	0	0	0.22	0.94	0.00	0.00
20	September	7	6	0	0	0.78	5.65	0.00	0.00
20	October	9	5	1	3	1.01	4.70	0.20	2.96
20	November	5	4	1	0	0.56	3.76	0.20	0.00
22	June	10	10	0	0	0.97	1.62	0.00	0.00
22	July	6	6	0	0	0.58	0.97	0.00	0.00
22	August	8	7	1	0	0.78	1.13	0.29	0.00
22	September	11	11	0	0	1.07	1.78	0.00	0.00
22	October	14	14	0	0	1.36	2.26	0.00	0.00
22	November	4	4	0	0	0.39	0.65	0.00	0.00
24	June	10	5	1	4	0.72	2.09	0.16	1.93
24	July	5	4	1	0	0.36	1.67	0.16	0.00
24	August	9	5	1	3	0.64	2.09	0.16	1.45
24	September	8	6	0	2	0.57	2.51	0.00	0.96
24	October	6	5	0	1	0.43	2.09	0.00	0.48

24	November	6	3	0	1	0.43	1.26	0.00	0.48
25	June	15	10	0	5	0.76	3.24	0.00	0.83
25	July	10	7	0	3	0.50	2.27	0.00	0.50
25	August	17	10	1	6	0.86	3.24	0.36	0.99
25	September	13	4	2	5	0.66	1.30	0.73	0.83
25	October	21	8	3	9	1.06	2.59	1.09	1.49
25	November	7	2	0	4	0.35	0.65	0.00	0.66

APPENDIX CC: City-Level Total Arrest Counts and Rates

Month	Counts				Rates per 10,000 population			
	All	Black	White	Hispanic	All	Black	White	Hispanic
June	7,951	5,700	670	1,472	29.30	67.98	7.67	32.84
July	7,266	5,192	652	1,310	26.78	61.92	7.47	29.23
August	6,805	4,897	666	1,146	25.08	58.40	7.63	25.57
September	7,044	5,101	591	1,237	25.96	60.83	6.77	27.60
October	6,742	4,887	586	1,174	24.85	58.28	6.71	26.19
November	6,321	4,718	510	990	23.30	56.27	5.84	22.09

District	Month	Counts				Rates per 10,000 population			
		All	Black	White	Hispanic	All	Black	White	Hispanic
1	June	299	204	52	39	42.23	139.02	14.82	140.50
1	July	314	212	56	42	44.35	144.47	15.96	151.31
1	August	293	222	47	20	41.38	151.28	13.39	72.05
1	September	317	230	47	37	44.77	156.73	13.39	133.30
1	October	310	227	43	33	43.78	154.69	12.25	118.89
1	November	269	204	33	31	37.99	139.02	9.40	111.68
2	June	362	345	6	11	37.74	52.68	3.43	63.78
2	July	264	241	10	11	27.52	36.80	5.72	63.78
2	August	238	223	9	6	24.81	34.05	5.15	34.79
2	September	272	248	11	11	28.36	37.87	6.29	63.78
2	October	287	271	8	7	29.92	41.38	4.57	40.58
2	November	240	225	7	9	25.02	34.35	4.00	52.18
3	June	459	457	1	3	58.37	64.03	3.15	63.88
3	July	347	332	5	8	44.13	46.52	15.74	170.33
3	August	307	306	2	2	39.04	42.88	6.30	42.58
3	September	310	308	3	2	39.42	43.16	9.45	42.58
3	October	262	256	7	2	33.32	35.87	22.04	42.58
3	November	262	260	3	2	33.32	36.43	9.45	42.58
4	June	375	307	13	55	31.50	42.01	12.80	20.17
4	July	343	284	4	53	28.81	38.86	3.94	19.43
4	August	281	245	11	28	23.60	33.53	10.83	10.27
4	September	281	248	9	25	23.60	33.94	8.86	9.17
4	October	338	270	13	56	28.39	36.95	12.80	20.53
4	November	308	256	11	40	25.87	35.03	10.83	14.67
5	June	418	409	5	7	58.80	61.40	39.26	44.41
5	July	417	402	5	13	58.66	60.35	39.26	82.47
5	August	400	388	9	6	56.27	58.25	70.66	38.07
5	September	410	398	8	7	57.68	59.75	62.81	44.41
5	October	340	332	6	4	47.83	49.84	47.11	25.38
5	November	370	365	3	4	52.05	54.79	23.55	25.38
6	June	420	412	6	3	47.58	48.43	72.24	126.56
6	July	411	401	7	5	46.56	47.13	84.28	210.93
6	August	403	396	5	3	45.65	46.55	60.20	126.56
6	September	433	430	3	1	49.05	50.54	36.12	42.19
6	October	488	482	2	6	55.28	56.66	24.08	253.12
6	November	454	447	9	1	51.43	52.54	108.36	42.19
7	June	567	554	11	4	90.69	94.41	178.53	91.19
7	July	516	509	3	4	82.53	86.74	48.69	91.19

7	August	426	409	5	14	68.14	69.70	81.15	319.17
7	September	564	548	8	10	90.21	93.39	129.84	227.98
7	October	495	481	5	11	79.17	81.97	81.15	250.77
7	November	390	380	3	10	62.38	64.76	48.69	227.98
8	June	386	195	44	144	15.34	38.92	9.19	22.17
8	July	394	215	34	143	15.65	42.91	7.10	22.02
8	August	359	169	63	127	14.26	33.73	13.15	19.56
8	September	386	174	45	163	15.34	34.73	9.39	25.10
8	October	344	178	42	119	13.67	35.53	8.77	18.32
8	November	313	159	42	112	12.44	31.74	8.77	17.25
9	June	427	157	40	221	25.67	94.40	16.77	40.27
9	July	396	162	37	196	23.81	97.41	15.51	35.71
9	August	393	163	52	172	23.63	98.01	21.80	31.34
9	September	407	162	45	195	24.47	97.41	18.86	35.53
9	October	356	138	32	186	21.40	82.98	13.41	33.89
9	November	276	123	20	131	16.59	73.96	8.38	23.87
10	June	443	252	13	179	40.77	73.33	36.61	38.22
10	July	412	230	25	160	37.92	66.93	70.41	34.17
10	August	405	262	17	127	37.28	76.24	47.88	27.12
10	September	450	270	24	152	41.42	78.57	67.59	32.46
10	October	425	263	15	146	39.12	76.53	42.25	31.18
10	November	451	334	9	106	41.51	97.20	25.35	22.64
11	June	970	849	55	58	135.45	142.55	275.45	206.77
11	July	936	814	68	55	130.70	136.67	340.56	196.08
11	August	890	773	54	56	124.28	129.79	270.44	199.64
11	September	983	869	50	61	137.26	145.90	250.41	217.47
11	October	799	717	37	45	111.57	120.38	185.30	160.43
11	November	778	703	39	31	108.64	118.03	195.32	110.52
12	June	260	144	46	66	19.52	60.14	8.20	29.06
12	July	233	134	41	56	17.49	55.96	7.31	24.65
12	August	209	126	28	56	15.69	52.62	4.99	24.65
12	September	196	120	31	44	14.71	50.11	5.53	19.37
12	October	163	92	27	43	12.24	38.42	4.81	18.93
12	November	190	122	26	41	14.26	50.95	4.64	18.05
14	June	207	54	25	126	17.01	63.06	4.46	35.69
14	July	171	41	30	96	14.05	47.88	5.36	27.19
14	August	167	48	22	90	13.72	56.06	3.93	25.49
14	September	136	39	22	70	11.17	45.55	3.93	19.83
14	October	140	44	27	69	11.50	51.38	4.82	19.54
14	November	125	32	34	58	10.27	37.37	6.07	16.43
15	June	496	464	16	19	84.36	86.50	130.61	221.98
15	July	507	475	16	17	86.23	88.55	130.61	198.62
15	August	440	401	18	22	74.84	74.75	146.93	257.03

15	September	383	362	6	17	65.14	67.48	48.98	198.62
15	October	383	362	7	15	65.14	67.48	57.14	175.25
15	November	454	430	10	15	77.22	80.16	81.63	175.25
16	June	184	49	82	52	8.99	186.27	6.04	18.30
16	July	185	36	88	56	9.04	136.85	6.48	19.71
16	August	167	36	76	47	8.16	136.85	5.60	16.54
16	September	169	22	86	50	8.26	83.63	6.33	17.60
16	October	160	36	75	46	7.82	136.85	5.52	16.19
16	November	145	30	62	45	7.08	114.04	4.57	15.84
17	June	157	23	47	81	10.50	52.70	8.21	19.06
17	July	120	29	33	53	8.03	66.45	5.77	12.47
17	August	124	21	32	65	8.29	48.12	5.59	15.29
17	September	141	34	28	78	9.43	77.91	4.89	18.35
17	October	124	21	30	66	8.29	48.12	5.24	15.53
17	November	113	18	37	53	7.56	41.25	6.47	12.47
18	June	226	142	55	31	18.45	143.11	6.06	64.48
18	July	172	111	38	19	14.04	111.86	4.19	39.52
18	August	230	170	37	21	18.77	171.32	4.08	43.68
18	September	180	127	36	17	14.69	127.99	3.97	35.36
18	October	209	144	46	19	17.06	145.12	5.07	39.52
18	November	196	131	28	33	16.00	132.02	3.08	68.64
19	June	330	193	75	54	15.85	152.84	4.79	34.09
19	July	214	104	59	51	10.28	82.36	3.77	32.20
19	August	233	113	77	43	11.19	89.48	4.92	27.15
19	September	220	111	55	48	10.57	87.90	3.51	30.30
19	October	233	93	83	50	11.19	73.65	5.30	31.57
19	November	212	92	68	51	10.18	72.85	4.34	32.20
20	June	81	33	19	28	9.08	31.05	3.87	27.58
20	July	87	37	22	20	9.75	34.81	4.48	19.70
20	August	65	28	21	16	7.29	26.35	4.28	15.76
20	September	67	34	11	21	7.51	31.99	2.24	20.69
20	October	80	38	20	21	8.97	35.76	4.08	20.69
20	November	85	44	17	24	9.53	41.40	3.47	23.64
22	June	220	206	12	3	21.33	33.33	3.42	12.30
22	July	205	186	20	2	19.87	30.09	5.70	8.20
22	August	184	168	14	5	17.84	27.18	3.99	20.50
22	September	184	170	11	4	17.84	27.50	3.14	16.40
22	October	235	219	16	3	22.78	35.43	4.56	12.30
22	November	163	156	8	1	15.80	25.24	2.28	4.10
24	June	168	87	30	46	12.04	36.42	4.84	22.17
24	July	126	74	23	22	9.03	30.98	3.71	10.60
24	August	129	64	36	25	9.24	26.79	5.81	12.05
24	September	119	62	21	32	8.53	25.96	3.39	15.42

24	October	162	85	30	45	11.61	35.59	4.84	21.68
24	November	138	73	28	29	9.89	30.56	4.52	13.97
25	June	496	186	39	264	25.02	60.26	14.16	43.64
25	July	496	185	50	250	25.02	59.93	18.15	41.32
25	August	462	188	53	217	23.30	60.91	19.24	35.87
25	September	436	157	53	214	21.99	50.86	19.24	35.37
25	October	409	160	37	204	20.63	51.84	13.43	33.72
25	November	389	156	35	185	19.62	50.54	12.71	30.58

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